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# The American Historical Review

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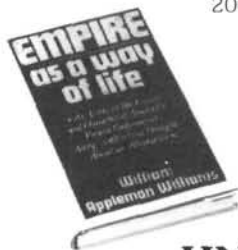
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*The American Historical Review* appears in February, April, June, October, and December of each year. It is published by the American Historical Association, 400 A Street, S.E., Washington, D.C. 20003 and is printed and mailed by the William Byrd Press, 2901 Byrdhill Road, Richmond, Virginia 23228. The editorial offices are located in Ballantine Hall, Indiana University, Bloomington, Indiana 47405.

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## Contributors

SHEARER DAVIS BOWMAN will receive his Ph.D. from the University of California at Berkeley in June 1981. A native of Richmond, Virginia, he earned his B.A. at the University of Virginia. He took an M.A. at Berkeley in the field of modern European history and then decided to shift his focus from Germany to the American South in the nineteenth century. The present article is based on his dissertation, written under the direction of Kenneth M. Stamp and Gerald D. Feldman.

RAYMOND GREW is a professor of history at the University of Michigan, where he has also served as the director of the Center for Western European Studies. He has been co-editor of *Comparative Studies in Society and History* since 1974. Although a specialist in modern French and Italian history, his most recent publications include "Modernization and Its Discontents," *American Behavioral Scientist*, 21 (1977), and his contributions to a book he edited, *Crises of Political Development in Europe and the United States* (1978). He received his college and graduate degrees from Harvard and is currently doing research on primary education in France and writing a book on the Catholic Church and social change in nineteenth-century France.

ALETTE OLIN HILL, after studying in Paris and Berlin, wrote her dissertation, "Sievers-Edgerton's Law and the Indo-European Semivowels in Greek," under the direction of George S. Lane at the University of North Caro-

lina. She has taught Sanskrit and linguistics at the University of Southern California and the University of Colorado. At Colorado she served as the director of the program in Innovative Education from 1975 to 1980. Her most recent publications are "The Carter Campaign in Retrospect: Decoding the Cartoons," *Semiotica*, 23 (1978), and "Hitler's Flag: A Case Study," *Semiotica* (forthcoming).

BOYD H. HILL, JR., is a professor of history at the University of Colorado, where he has served as the resident director of a national Summer Institute in Basic Disciplines (Diplomatics and Paleography), sponsored by the Mediaeval Academy of America in 1971. He has been a member of the council of the Pacific Coast Branch, American Historical Association (1971-73), and of the Mediaeval Academy of America (1973-76). Among his publications are "The Grain and the Spirit in Mediaeval Anatomy," *Speculum*, 40 (1965), and *Medieval Monarchy in Action: The German Empire from Henry I to Henry IV* (1972).

PETER KOLCHIN, an associate professor of history at the University of New Mexico, is working on a comparative study of American slavery and Russian serfdom. He is the author of *First Freedom: The Responses of Alabama's Blacks to Emancipation and Reconstruction* (1972), "The Process of Confrontation: Patterns of Bondage in Nineteenth-Century Russia and the United States," *Journal of Social History*, 11 (1977-78), and other articles.

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# The Case for Comparing Histories

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RAYMOND GREW

HISTORICAL COMPARISON HAS BEEN WIDELY PRAISED by many of the best minds of the historical profession. Marc Bloch, whose prestige and influence have increased since his death, called for historians to work comparatively in a famous article published over fifty years ago,<sup>1</sup> and Lord Acton argued for historical comparison fifty years before that.<sup>2</sup> Indeed, the pedigree of that approach could be extended back through William Robertson and Adam Smith to Montesquieu, Vico, and Machiavelli and on to Polybius and Herodotus. Auguste Comte, that often disowned father of social science, considered comparison to be the highest form of observation and, when undertaken through “the historical method,” believed it “the only basis on which the system of political logic can rest” as well as the method that would provide “philosophic character to sociology in a logical as well as a scientific sense.”<sup>3</sup> And Marxism, perhaps even more today than in the past, is a powerful stimulus to historical comparison.

Admittedly, however, for many professional historians comparative study evokes the ambivalence of a good bourgeois toward the best wines: to appreciate them is a sign of good taste, but indulgence seems a little loose and wasteful. In part, such hesitance reflects some of the admirable if modest qualities most widely respected and fully shared within the historical profession—caution, accuracy, unpretentiousness, and respect for the integrity of documents and for the particular. In part, it reflects doubt not so much about comparison as a mode of analysis as about what it is that historians should compare. For most historians, the models that come readily to mind are troubling.

An earlier version of this article was presented as a paper at the Ninety-Third Annual Meeting of the American Historical Association, held in San Francisco, December 1978. I am grateful to a number of colleagues—especially Marvin Becker, David Bien, Robin Jacoby, and Sylvia Thrupp—for stimulating criticism.

<sup>1</sup> Bloch, “Pour une histoire comparée des sociétés européennes,” paper delivered at the Sixth International Congress of Historical Sciences, held in Oslo, August 1928, printed in the *Revue de synthèse historique*, 46 (1928): 15–50, and reprinted in Bloch, *Mélanges historiques*, 1 (Paris, 1963): 16–40. For an English version, see Bloch, “Toward a Comparative History of European Societies,” trans. Jelle C. Riemersma, in Frederic C. Lane and Jelle C. Riemersma, eds., *Enterprise and Secular Change: Readings in Economic History* (Homewood, Ill., 1953), 494–521.

<sup>2</sup> “The process of Civilization depends on transcending Nationality. Everything is tried by more courts, before a larger audience. Comparative methods are applied. Influences which are accidental yield to those which are rational.” Note of Lord Acton, University of Michigan, Add. MSS 4908. Sylvia Thrupp used Acton’s statement to begin the first issue of *Comparative Studies in Society and History: An International Journal* in October 1958.

<sup>3</sup> Comte, *Cours de philosophie positive*, in *Auguste Comte and Positivism: The Essential Writings*, ed. Gertrud Lenzer (New York, 1975), 247, 249.

"COMPARATIVE HISTORY"—a term so compromised that, throughout the program for the Ninety-Third Annual Meeting of the American Historical Association in December 1978, it was properly wrapped in quotation marks—suggests first of all the comparison of civilizations, comparison on the grandest scale in the manner of Spengler, Sorokin, and Toynbee. These great efforts, themselves part of Europe's encounter with the rest of the globe in the era of two world wars, remain stimulating and provocative for many. But their search for the morphology of history is not intimately related to what most practicing historians really do, very few of whom would agree that "the most important task of the scientific study of history will be the comparative study of the development of whole civilizations." In his intelligent book Philip Bagby went on to declare, "The actual technique of describing cultural ideas and values is a fairly simple one. It consists of listing all or, at least, a number of the forms of cultural behaviour, the speech, and actions in which they are expressed, and the artifacts which result from such behaviour."<sup>4</sup> And any reader knows immediately that this student of A. L. Kroeber is not a historian.

When civilizations are compared primarily in terms of certain central topics, the historian is on more familiar ground. Comparison, then, as in the *grands thèmes* of international historical congresses, merely treats familiar concerns on a broader scale. Comparative religion or the relations of state and society are subjects of great classics of historical literature, especially in German, and, even when segregated as philosophy of history, such sweeping interests have animated whole schools of study. If as historians we like to call these approaches philosophy, it is because they seem, even after more than a century of remarkable works that deny the distinction, to rest less on what historians call research than on what the very learned already know (in short, on the state of our culture), on the wise discernment of patterns, and on the generation of fresh hypotheses.

A third well-recognized mode of comparison treats not whole civilizations or major aspects of them but historical processes. These can range enormously in scope, from comparative study of the "alternative modes of production"<sup>5</sup> to the pace and degree of industrialization in a single industry; from the comparison of revolutions,<sup>6</sup> a subject of undying popularity, to the comparison of processes still more closely defined, such as the spread of modern educational systems.<sup>7</sup> This sort of historical comparison is especially congenial to economics, sociology, and some schools of anthropology; and, as with the now much-criticized concept of "modernization," such comparisons lead to exchanges of methods and data, in-

<sup>4</sup> Bagby, *Culture and History: Prolegomena to the Comparative Study of Civilizations* (Berkeley and Los Angeles, 1963), 190, 192.

<sup>5</sup> Immanuel M. Wallerstein has called for this sort of comparative study; see his "Modernization: Requiescat in Pace," in L. Coser and O. Larson, eds., *The Uses of Controversy in Sociology* (New York, 1976), as quoted in Barbara Hockey Kaplan, ed., *Introduction to Social Change in the Capitalist World Economy* (Beverly Hills, 1978), 10–11.

<sup>6</sup> For some recent approaches, see the articles by Perez Zagorin, Theda Skocpol, and Elbaki Hermassi in *Comparative Studies in Society and History*, 18 (1976).

<sup>7</sup> See Michalina Vaughan and Margaret Scott Archer, *Social Conflict and Educational Change in England and France, 1789–1848* (Cambridge, 1971).



terest in common problems, and a shared vocabulary that have lasting benefits.<sup>8</sup> Many of the most often-cited works of recent historical comparison belong in this category, although, significantly enough, most have not been written by scholars professionally trained as historians.<sup>9</sup>

Yet the comparison of historical processes also evokes resistance, even suspicion, among many historians. Much of that suspicion may result from methodological naiveté, intuitive nominalism, or sheer prejudice; but weightier concerns informed by an awareness of the common nineteenth-century roots of all of the social sciences also seem to be involved. Nothing is more awkwardly old-fashioned than optimism no longer felt—in this case about the nature of social knowledge, the validity of universal laws, and the meaning of history. Furthermore, our professional distaste for easy determinism deserves serious consideration on methodological grounds. Historical determinism has been one of the great spurs to comparison among Hegelians and materialists and from Buckle, Tylor, Spencer, Maine, and Morgan to the present day,<sup>10</sup> for determinism implies the existence of destinies and definable laws that comparison should be able to uncover. But determinism can also place an early limit on the power of comparison to uncover surprises or display variety and, thereby, to redefine traditional problems.

The nineteenth-century evolutionists tended in practice to compare stages of evolution or other analytic abstractions, and this dependence on definition is further reason for historians' discomfort. Sensitive to the differences that chronology makes, historians are bound to wonder whether a process remains the same when it lasts for decades in one instance and for centuries in another or when it occurs in different eras altogether. Are two sets of events comparable because both are called revolutions or are the crises, revolutions, and cycles assembled for analysis mere homonyms (a kind of comparison that Marc Bloch warned against), a humorless misunderstanding of a historical pun? The answer, of course, is that the validity of any comparison rests on careful definitions against which the elements compared must be systematically tested. This obvious methodological solution explains why so much of the effort in works comparing historical process is taxonomic, why so much of the actual comparison is of categories and classifications.

These problems are reduced if institutions are the unit of comparison; and much of the best work, especially in premodern history, is of this sort. But institutions that go by common names of church or party or bank may, in fact, perform quite different functions in different societies or at different times. Structural-functionalism in anthropology and sociology has helped clarify this

<sup>8</sup> For some assessments of the continuing debate on modernization and its implications for comparison, see the April 1978 issue of *Comparative Studies in Society and History*, which was devoted to that question, and note the earlier articles mentioned in the Editorial Foreword, *ibid.*

<sup>9</sup> Among others, see the works of S. N. Eisenstadt, Michel Foucault, Samuel P. Huntington, Barrington Moore, Charles Tilly, and Immanuel Wallerstein and of the Committee on Comparative Politics of the Social Science Research Council.

<sup>10</sup> See J. W. Burrow, *Evolution and Society: A Study of Victorian Social Thought* (Cambridge, 1966); and Maurice Mandelbaum, *History, Man, & Reason: A Study in Nineteenth-Century Thought* (Baltimore, 1971).

problem and has been effectively used especially by historians of political parties and economic development.<sup>11</sup> Ultimately, however, social functions, like most analytic abstractions, raise for historians the taxonomic discomfort of categories that seem to lie outside the field of study (the workday historian is likely to complain of “imposing” something on the past). The emphasis on structural analysis now more in vogue lessens the abstractness, for structures are more easily tied to specific behavior. Studies of the crowd, for example, were received into the historical canon primarily as ideology<sup>12</sup> until Marxian concern for class differences led historians to seek a firm relationship among particular crowds, their behavior, and specific conditions. Then the comparison of crowds became especially fruitful. Yet the search for structures is vulnerable to a subtle determinism that invites merely secondary interest in *mentalités* or cultural values, and historians are wisely sensitive to modes of analysis that too readily limit or exclude. If the comparison of processes calls attention to the problems of definition, so the comparison of structures calls attention to the dangers of abstractness.

Methodological sophistication in matters of morphology and taxonomy or typology can be of great value in historical analysis, and I assume no historian so benighted as to denounce the effort in advance would have chosen to read this far. But such analytic tools require—it is their very purpose—fitting bits of history into categories already established, and conceptually that is uncongenial for most historians, even though in practice we do this all the time. It may not be more “artificial” to define a topic in terms of the available historical sources, traditional political boundaries, or conventional chronology; but historians, like everyone else, notice more readily that to which they are unaccustomed. To start with fixed categories is to admit to approaching history from the outside. To the historically sensitive, however great their gift for theory, this is not a comfortable way to work. “Events strikingly similar but occurring in a different historical milieu,” wrote Karl Marx in 1877, “lead to completely different results.” And, he added, “By studying each of these evolutions separately and then comparing them, it is easier to find the key to the understanding of this phenomenon; but it is never possible to arrive at this understanding by using the *passe-partout* of some universal historical-philosophical theory whose great virtue is to stand above history.”<sup>13</sup> The common impression that historical comparison requires just such an approach—that it consists in comparing groups or events, institutions or ideas that in the abstract have been labeled as comparable—may in large part account for the scarcity of systematic comparison in historical writing. Significantly, the comparisons most admired and accepted tend to be of the sort that emerge in the works of Jacob Burckhardt, R. R. Pal-

<sup>11</sup> S. N. Eisenstadt has seen both Claude Lévi-Strauss and Talcott Parsons, however, as providing bases for the comparison of types; Eisenstadt, *Essays on Comparative Institutions* (New York, 1965), 47. Parsons has conveyed a view of history remarkably similar to the grand histories of civilization; see his *Societies: Evolutionary and Comparative Perspectives* (Englewood Cliffs, N.J., 1966) and *The System of Modern Societies* (Englewood Cliffs, N.J., 1971).

<sup>12</sup> Or they were received as works that could safely be ignored, as in the case of the striking *aperçus* of Elias Canetti; see his *Crowds and Power*, trans. Carol Stewart (New York, 1972).

<sup>13</sup> Marx, Letter published in a Russian newspaper, as quoted in Edward Hallett Carr, *What Is History?* (New York, 1961), 82.

mer, or Fernand Braudel, studies whose scope was not determined by categories of comparison but that used a framework already familiar and acceptable and also large enough to include space for comparative analysis.

The willingness to compare societies rests, most attractively, on the assumption of a certain universality in the human condition, on the belief that, just as individuals and societies can learn from each other, we can learn by comparing human behavior in similar or different contexts. One of the more admirable of our own cultural traditions is thus reinforced in our times by easier communication and the methods of social science. And I believe one sees an increasing tendency among Western scholars to acknowledge, at least in their footnotes, the relevance of the African or Indian or Chinese experience to the understanding of our own behavior. Heirs of the nineteenth century, when national differences seemed fundamental, and nowadays surrounded by an intense ethnic consciousness, we have a special need for cross-national comparisons. Significantly, they remain more common in medieval than in modern history, in the study of Europe than in that of the United States. In the writing of American history it is surprising, as John Higham has commented, not that comparison "should at last have begun to flourish, but that it should have been so long delayed."<sup>14</sup> So great, then, is the need that "comparative history" is often taken to mean comparison between nations,<sup>15</sup> which has the paradoxical effect of reinforcing the tendency to consider nations the major unit of analysis. That, in turn, has discouraged comparison by those who prefer to work in primary sources; for it makes comparison seem to require an equally intimate knowledge of at least two societies, two languages, two distinct traditions of record-keeping and interpretation. Few historians are willing to abandon the benefits of a specialization that has enormously expanded historical knowledge and sophistication.<sup>16</sup> It is therefore important to stress that many of the benefits of comparison can result from studies in which all of the cases fall within a single nation but exemplify differences in time or space or social group or even in which the cases compared have not been investigated at equal depth. Comparative analysis, like the architect's compass, is no less useful a tool of design when the legs on which it stands are of unequal length.

<sup>14</sup> Higham, *Writing American History: Essays on Modern Scholarship* (Bloomington, Ind., 1970), 165. He has suggested that the vigorous effort in American studies to be interdisciplinary may have got in the way of inter-cultural analysis. Like so many others, historians of the United States have been transfixed by the national state. Thus, comparative analysis is central to the historiography of the colonial period but uncommon thereafter except for certain classic questions like slavery (which we would like to see as foreign anyway) and such challenges to the state as regionalism and civil war.

<sup>15</sup> For a discussion of the development and difficulty of such ambitions, see Stein Rokkan, "Comparative Cross-National Research: The Context of Current Efforts," in Richard L. Merritt and Stein Rokkan, eds., *Comparing Nations: The Use of Quantitative Data in Cross-National Research* (New Haven, 1966), 3-25. Terence Hopkins and Immanuel Wallerstein have distinguished three kinds of "plurinational" study, but David S. Landes and Charles Tilly have seen comparison more broadly and predicted a central place for it within the historical discipline; see Hopkins and Wallerstein, "The Comparative Study of National Societies," reprinted in Amitai Etzioni and Frederic L. Dubow, eds., *Comparative Perspectives: Theories and Methods* (Boston, 1970), 183-204; and Landes and Tilly, eds., *History as Social Science* (Englewood Cliffs, N.J., 1971).

<sup>16</sup> The benefits of using primary materials are often misstated. In practice, greater historical understanding is threatened more by mindlessness than by error, for inaccuracies are easier to identify and correct than are misleading assumptions. The great advantage of working from primary sources is less any increased assurance that the facts are right than the increased opportunity for independent, original analysis that is at the same time sensitive to historical context.

BECAUSE, AMONG PROFESSIONAL HISTORIANS, COMPARISON is more widely admired than consciously practiced, it may be useful to set forth a fairly simple and unpretentious case for thinking about history comparatively, one that need not arouse inherited doubts and suspicions. Some of the simpler advantages of comparison are more apparent in the other social sciences. For those dealing with contemporary issues in another society, the dangers of ethnocentrism, mistranslation of culturally imbedded meanings, and false assumptions of comparability are immediate. Historians—usually analyzing a world different from the one in which they live, experienced in the culture they study, and guided by scholarly (and cultural) lore—often avoid the most blatant forms of these errors without once shuddering at the peril they missed. And when, a generation later, the historian in turn is recognized to have reflected his own times, history—the most cumulative of the social sciences in knowledge if not in method—seems hardly to have been harmed. Yet comparison, for the historian too, can help prevent the misinterpretation of other cultures.<sup>17</sup>

That comparison is central to any scientific method has become a truism;<sup>18</sup> and logicians like to point out that to declare anything unique, supposedly a penchant of historians, is by implication to compare it to a class of things to which it purportedly belongs. All historical narrative rests on some assumptions as to what constitutes normal behavior and what others have done in similar circumstances. In addition, the most consistently fruitful source of new insight into the past is probably the historian's own experience of the present. (How revealing it is that we speak of a scholar's gaining or providing a fresh perspective.) Striving for objectivity in our finished analyses, we tend to admit our grounding in the present a bit shamefacedly; but it can, like hindsight or the fact that the historian is always something of an outsider, be a welcome asset, an aid to good comparison. Similarly, the historian's preoccupation with change is a commitment to comparison. Thus, the need to compare and the habit of doing it are a large part of the practice that makes history a discipline, that enables an "experienced" historian to judge a piece of scholarship in a distant field of history. This is done by assessing the significance of the problem posed, the importance of the argument made, and the quality of the evidence presented largely by a series of comparisons both with other scholarly works and with other sets of historical problems. In short, history can be substituted for sociology in Émile Durkheim's dictum, "Comparative sociology is not a particular

<sup>17</sup> Maurice Mandelbaum has made this point, significantly adding that this "need not involve an abandonment of the historian's ideographic approach"; Mandelbaum, *The Anatomy of Historical Knowledge* (Baltimore, 1977), 14. And Barrington Moore, Jr., has noted that comparisons "can lead to asking very useful and sometimes new questions . . . , can serve as a rough check on accepted historical explanations," and "may lead to new historical generalizations"; Moore, *Social Origins of Dictatorship and Democracy: Lord and Peasant in the Making of the Modern World* (Boston, 1966), xiii.

<sup>18</sup> In his admirable discussion of Claude Lévi-Strauss and Talcott Parsons, Guy E. Swanson began by noting that all research entails comparison and pungently added, "Thinking without comparison is unthinkable"; Swanson, "Frameworks for Comparative Research: Structural Anthropology and the Theory of Action," in Ivan Vallier, ed., *Comparative Methods in Sociology* (Berkeley and Los Angeles, 1971), 141, 145.

branch of sociology; it is sociology itself, in so far as it ceases to be purely descriptive and aspires to account for the facts."<sup>19</sup>

In the broadest sense, then, comparison is unavoidable. The question is not so much whether historians should make comparisons but whether the study of history benefits when those comparisons are made consciously and sometimes even systematically. Deliberately used, comparison can aid historians at four stages of their work: (1) in asking questions, (2) in identifying historical problems, (3) in designing the appropriate research, and (4) in reaching and testing significant conclusions.

In even a single piece of research, the historian may use many quite different comparisons in quite different ways. First, the initial stage of truly original historical research is the focused curiosity that comes with the asking of fresh questions. And comparison is probably their most consistent source, suggesting by examples that what looks like change may be continuity or that things seemingly unrelated may be connected. To look at other cases is to see other outcomes. By considering them, the historian wins some freedom from the tyranny of what happened and develops that awareness of alternatives—of a missing revolution, of banks that were not formed, of parliaments that did not meet, of populations that failed to increase—that underlies some of the most provocative of historical questions. We are easily blinded by the obvious. As Bloch noted of German and French ways of dividing property in the Middle Ages, either pattern studied by itself would seem natural; only comparison establishes that there is something to be explained.<sup>20</sup> When the historiography of one time and place develops a rich set of analytic categories, their very effectiveness tends to foreclose “inapplicable” questions—until, that is, the historical imagination is stimulated anew by the surprises that follow from applying to familiar issues some categories of questions evolved in another context. No less a thinker than Charles Darwin, one of the most effective users of comparison, expressed admiration for the brilliance of Henry Thomas Buckle (although soon adding, “I doubt that his generalizations are worth anything”),<sup>21</sup> and we should consider the nature of that brilliance. Confident in historical laws, Buckle was eager to seek evidence everywhere and so willing to rub one fact against another that he expanded the range of historical curiosity to touch in a few pages on climate and crime, ballads, philology, the price of wheat, the influence of the clergy, the causes of dueling, the impact of the theater, and the importance of colonies. When comparison has uncovered interrelationships usually overlooked, its effect

<sup>19</sup> Durkheim, *Rules of Sociological Method*, ed. E. F. Catlin, trans. Sarah A. Solovay and John H. Mueller (Glencoe, Ill., 1964), 139. Daniel P. Warick and Samuel Osheron have discussed the uses of comparison and noted the debate in sociology as to whether comparative sociology is a separate field. They decided it is not, citing Oscar Lewis's comment, “Comparison is a generic aspect of human thought rather than a special method.” Warick and Osheron, “Comparative Analysis in the Social Sciences,” in Daniel P. Warick and Samuel Osheron, eds., *Comparative Research Methods* (Englewood Cliffs, N.J., 1973), 6–11.

<sup>20</sup> Bloch, “Pour une histoire comparée,” 39.

<sup>21</sup> Buckle's biographer in turn attributed Darwin's skepticism to “a certain genial density”; John MacKinnon Robinson, *Buckle and His Critics* (London, 1895), 25; and Buckle, *Miscellaneous and Posthumous Works*, 1 (London, 1872).



is also to increase awareness of the differences between cases—a result often more fruitful than the initial impression of similarities. In all of these ways, comparison can lead to good historical questions. Now it may not matter how these questions are arrived at or what prejudices or misconceptions have stimulated this new awareness; but most of us need more than a falling apple. In adopting historical comparison we can make use of, for our purposes, the best thought in other fields.<sup>22</sup>

The purpose of posing questions is to identify historical problems that merit further research, a second stage of the historian's work. Once certain clusters of questions are identified as interrelated in ways that constitute a problem or problems for which solutions should be sought, comparison can be used with somewhat greater deliberation to determine which historical elements from an infinite variety ought to be included. Here, too, comparison is a means to breaking out of the trenches dug by received conceptions, but it should bring to bear the experience of other times and places and the work of other scholars in defining the unit of study—justifying, in effect, these particular cuts across history's seamless web. Conceived comparatively, the historical problem is then more likely to determine the scope of the research—investigation that may require chronological, geographical, and social dimensions that are quite unconventional. (Acton and Bloch particularly stressed the need to use comparison to overcome the national particularism built into European historiography.) It is important to understand that the first essential of sound comparison is well-defined categories of analysis and that these, simply by being too broad or too narrow for the problem at hand, can make it difficult to uncover anything new through comparison.

In a third stage, the establishment of a research design, comparison can be used to suggest by example the applicability of evidence and methods that might otherwise be overlooked, although they have worked well elsewhere, and to search for critical variables. As most of its advocates have insisted, systematic historical comparison should often stimulate new attention to the details of individual cases and to local studies.<sup>23</sup> Truly original historical analysis usually strives to build its arguments from the inside out, and we should not allow the daring use of sweeping comparisons (more often ventured in our sister social sciences) to create a misimpression that comparison only moves from the abstract to the particular. To ask historians to work comparatively is not to uproot them from their sources (nor to embrace the desiccation that would be likely to fol-

<sup>22</sup> Hugh Stretton has argued for comparison as a means of extending the investigator's experience and stimulating the imagination; but Arend Lijphart has criticized this position in favor of the conventional view that comparison serves primarily to test hypotheses; see Stretton, *The Political Sciences: General Principles of Selection in Social Science and History* (London, 1969), 69, 245–47; and Lijphart, "The Comparable Case Strategy in Comparative Research," *Comparative Political Studies*, 8 (July 1975): 159–60.

<sup>23</sup> See Bloch, of course, "Pour une histoire comparée," 40. But Lijphart has cited such sociologists as Neil J. Smelser and Juan Linz to the same effect; "Comparative Research," 67–68. And the list could easily be extended. Arthur L. Stinchcombe has provided an extended argument rejecting the idea of some tension between facts and generalization, declaring that "it is the details that theories in history have to grasp if they are to be any good" and that "social theory without attention to detail is wind"; Stinchcombe, *Theoretical Methods in Social History* (New York, 1978), 115–16, 124, 21–22.



low). Whereas concern for detailed accuracy can bury significance under trivia, comparison can make the apparently trivial significant—an asset few of us can afford to ignore. As the problem is defined and a design for research established, strategies of comparison become more formal. Proper concern for the complexity of a particular context may invite comparison of its major elements with many different cases; the task is then to see wholes and compare parts<sup>24</sup> (for all of the majesty of his effort, Arnold Toynbee often exemplified the dangers of doing the reverse).

When research leads to the recognition of general patterns, formal hypotheses, or full-fledged theories, these can again be tested by comparison—this time in ways as methodologically systematic as possible. This fourth and most formal stage of comparison is the one most written about in works on method. But comparison can be claimed to be the historian's (or the social scientist's) form of experiment in the multiple tasks of preliminary soundings and research design as well as verification. These multiple uses of comparison form a continuum, as do the differences in scale from one instance seen comparatively to many and from comparisons within one society to those that move across countries, cultures, or time. The value of any historical comparison rests not on the importance of the cases compared or on their number or scope but on the quality of the discernment, of the research, and of the general statements that result.

This is not to say that all valuable historical work must be consciously comparative. The absolutely essential effort to determine what happened and in what sequence or to assess the validity of suspect evidence need not involve historical comparison, except in the broad sense that judgment always has some comparative elements. Nor is the scholar working on a single case so likely to use comparison when the problems posed have been fully set by historiographical tradition. Invaluable contributions have, nevertheless, been made in these ways.

The case for comparison should conclude, however, by noting that the various levels and kinds of conscious historical comparison contribute to the endless task of building a science of society, and they do so most importantly in a relatively straightforward way. The slow, concrete construction of a common vocabulary enables students of different societies working from different disciplines to speak to each other.<sup>25</sup> Sometimes we can even work from a shared set of questions to create the kind of dialogue that is the essence of science. We must not claim too much, for the projects devoted to interdisciplinary research during the last thirty years give reason for modesty as well as hope. Neither need we reject

<sup>24</sup> Robert F. Berkhofer, Jr., has argued that in a final sense a reconciliation of the two approaches so far appears "analytically unattainable," but his standards are very high; in any case, the *effort* seems central to much of the best historical writing. See Berkhofer, "Clio and the Culture Concept: Some Impressions of a Changing Relationship in American Historiography," in Louis Schneider and Charles M. Bonsean, eds., *Culture in the Social Sciences* (Cambridge, 1973), 99–100. Guy Swanson has cited Lévi-Strauss to the effect that the "comparative method consists precisely in integrating a particular phenomenon into a larger whole" and that research should be "so conducted that theory is at every point in touch with observations"; "Comparative Research," 146.

<sup>25</sup> For a well-developed case for this sort of cross-disciplinary work, facilitated by the "federated" nature of American academic departments, see Bernard S. Cohn, "History and Political Science," in Otto von Mering and Leonard Kasdan, eds., *Anthropology and the Behavioral and Health Sciences* (Pittsburgh, 1970), 89–111.

the vision of working to establish a common questionnaire,<sup>26</sup> of sharing the excitement of the hunt as well as the published reports of the kill.

IN LIGHT OF SUCH BROAD YET GENTLE CLAIMS for the benefits of comparison, it is possible to say something about when historical comparison works best. For instance, it is my impression—to select two fields safely distant—that the results of comparison have been more striking and consistent in the history of art than in the history of literature. Literature, like history itself, is so easily understood as encompassing most of life that problems of focus and selectivity can be overwhelming, reinforcing a tendency we all share to let the traditions of academic monographs rather than independent analysis shape our topics. Yet there are many works of comparative literature every historian might envy. If most of us cannot assume the brilliance or adopt the erudition of Renato Poggioli or Northrop Frye,<sup>27</sup> we can learn from the skill with which they use the concept of established literary forms (much as historians use the idea of institutions) and mastery of rhetoric (much as sociologists use methodology). In only one respect do they enjoy an inherent advantage denied most historians: they may assume that their readers are already familiar with some of their primary sources. At more ordinary levels of scholarship, however, comparison in art history seems more often to have led to new discoveries. Perhaps because painting and sculpture, which are more removed from common discourse, are more abstractly understood, because the artistic medium itself is more expressly limiting yet universal, and because its functions and symbolic structure are more formal, the elements of comparison can be more readily defined with regard to art than to literature.

Or, closer to home, the comparisons of national character, once so fashionable, have been largely abandoned because too many different things were compared at once, leaving rules of evidence so loose that the relationship of doubtful assumption and descriptive example was circular. Demography, on the other hand, enjoys its current triumph in large part because universal elements in human biology and numerous cases permit the supreme comparability and subtle analysis of quantification, with which it is possible to distinguish significant social differences. Nevertheless, historical studies of the family have proved effective when focused on particular, well-defined problems, less so when too broadly aimed.

Similarly, the magnificent sweep of Jungian categories is only on occasion and with difficulty tied to the kind of evidence with which historians deal. Psychohistorical studies of individuals, however, are likely to leap from the abso-

<sup>26</sup> The term is once again Marc Bloch's, who meant by it the set of questions a Continental judge puts to a witness rather than a form to be filled out or a tool of survey research.

<sup>27</sup> See, for example, Frye, *Anatomy of Criticism* (New York, 1967); and Poggioli, *The Theory of the Avant-Garde*, trans. Gerald Fitzgerald (Cambridge, 1968). Fitzgerald commented in his "Translator's Apology" that "it is this assembly of points into patterns that makes his [Poggioli's] book such a stimulant and a pleasure"; *ibid.*, ix. Studies of political cartoons have offered a striking use of comparison in *Comparative Studies in Society and History*.

lutely particular to Freudian categories that group many cases without necessarily inviting historical comparison. When, however, psychological analysis points to social roles in specific contexts (families, generations, children, or true believers), then very fruitful historical comparisons can follow.

On the whole, historical comparison seems most effective at a kind of middle range.<sup>28</sup> The term is imprecise, but obviously comparison is most enlightening when the choice of what to compare is made in terms of general and significant problems, the elements compared are clearly distinguished, and attention is paid to the intricate relationships between the elements compared and the particular societies in which they are located. These criteria are most likely to be met when there are models or theories that can be concretely applied, when the evidence is extensive and rooted in its historical context (which often means that it has been generated with just these problems in view), and when the cases are delimited. Then one seeks explanations and generalizations but not universal laws. The search is for patterns of behavior and circumscribed hypotheses, and it is as likely to result in the recognition of unexpected connections between aspects of society previously thought to be unrelated as in general theory. In practice, the importance of such findings will more often be measured by the stimulus they give to subsequent research than by formal validation.

As a faintly empirical appraisal of the sort of work currently being done in this middle range, I have attempted to analyze the last five hundred manuscripts submitted to the quarterly journal *Comparative Studies in Society and History*. Obviously, any single publication develops its own traditions and is unlikely to be very precisely representative; furthermore, although these five hundred authors come from some thirty countries, nearly two-thirds of them either are citizens of the United States or are teaching at American universities. Among these manuscripts, there is surprisingly little difference between the kinds of topics chosen by historians and those favored by sociologists, political scientists, and anthropologists. Nor is there, incidentally, any general difference between the topics of that small fraction of manuscripts selected for publication and all of the rest: the printed articles tend simply to provide more carefully developed arguments (or models) and to rest on more extensive and suggestive research. Paradoxically, the ones rejected are often among the most deliberately comparative, turned down because readers felt a rather mechanical application of someone else's categories had added little either to general theory or to the understanding of particular cases.

For what it is worth, I think I see in these five hundred manuscripts support for seven general statements about the current practice of historical comparison.

1. Matters colonial constitute much the favorite subject matter, a sobering reminder of the extent to which all of the social sciences are influenced by contemporary politics and of how much the impe-

<sup>28</sup> This echoes, of course, Robert K. Merton, "Theories of the Middle Range," in his *Social Theory and Social Structure* (New York, 1952). Richard H. Brown has suggested, however, that Merton merely meant testable theories; Brown, *A Poetic for Sociology* (Cambridge, 1977), 13.

tus to comparative study was stimulated by the West's imperial encounters with other societies (although Greece and Rome, ancient Egypt, and China have exercised a similar attraction). And a more intrinsic reason probably underlies this emphasis: the colonial experience offers a degree of analytic control not usually available to social scientists; new influences and pressures can be identified and their assimilation, distortion, or rejection can be traced. One can, perhaps with misleading ease, identify the source of social change. A similar set of advantages, I think, makes the study of ethnic or religious minorities and of migration popular; the subject may appeal to academic sensibilities, but the cases themselves, while speaking to the most universal social issues, have a closed quality that allows a distinction between internally generated and externally stimulated change and that makes more visible the confluence of old customs and ideologies with new social and political needs (the complex experience of Israel may have stimulated more good comparative study than that of any other modern nation). In historical comparison, cases that in themselves allow a kind of experimental control, that segregate certain variables from the larger social flow, offer particular advantages.

2. Articles on currently fashionable topics (such as women's history, birth control, crime, terrorism, or social services) are so frequent as to suggest something more than the latest style; comparative analysis of historical cases is especially valuable in building new sets of questions into a field of study and in connecting these questions to the established literature. Social history in general has been associated with comparative study for many reasons. It has emphasized the material circumstances that condition people's lives, and these are relatively easy to compare. It has sought to analyze social structures and, in doing so, has effectively used categories, concepts, and theories developed first by economists, sociologists, and anthropologists, making comparison at least implicit at every stage of research. Social history has welcomed quantitative methods, which facilitate comparison (and also its misuse); and relating specific phenomena to a larger society is an intrinsically comparative effort. Social historians, attentive to classes that have not left a rich, written record of their culture, are readily pushed to comparison when little encumbered by knowledge of the complexity of the lives led by anonymous people who have been seen from the first as an abstract type. Most simply, social historians have welcomed comparison as they saw themselves building a new subdiscipline. This may well mean that attention to comparison within social history will decline as it becomes an established field, an increasing number of whose scholars will choose to pursue in a single context questions already well developed.

3. Whenever a body of theory is already well established, it becomes easier to discern the significant elements of a particular case; empirical studies of slavery, millenarianism, land tenure, or revolution, for example, thereby gain strength. The use of theory has also become more sophisticated, and the suggestion that some general theory exists, which might be applied to real social experiences, no longer is likely in itself to seem exciting (another reason comparative study is needed). Rather, theoretical ideas are used more familiarly to illumine specific research and in somewhat scholastic logical exercises.
4. Comparative study offers a special stimulus to—and benefits by—relating categories of analysis usually left separate: peasants or workers and the larger culture, ethnic consciousness and economic structure are examples. For all of these reasons, historical comparison seems likely to flourish in the next decade or two among those analyzing the intersection of social structure and culture, the relationship of high to popular culture, and the employment of economic and political power in international relations. Important theoretical questions are at stake in each topic, and concrete comparative analysis is needed to determine where the critical connections and problems are.
5. Most of the articles submitted to *Comparative Studies in Society and History* give full attention only to a single historical case; the next largest group treats several cases across time but within a single society, and such articles are followed by those that are essentially theoretical. The minority that give equal attention to cases from several different societies are not necessarily those that most effectively use comparison to make new discoveries.
6. Although most of the authors submitting manuscripts seem to believe that social structures are more important than social attitudes, or at least are more respectable as a subject for analysis, attitudes and ideas frequently receive more space. Many explanations leap to mind, but it is possible that, whereas our vocabulary for the analysis of social structures tends to underscore the similarities between cases, it is the differences (which are more readily expressed in terms of attitudes) that are more interesting.
7. Finally, the established interests of social scientists—mobility, political behavior, law, education, religious movements, and, even more suspect, modernization or revolution—can still, through systematic comparisons, provide the occasion for fresh and exciting new understanding.

THESE CONSIDERATIONS OF COMPARATIVE STUDY IN HISTORY—of the ways in which it is commonly understood and the somewhat different ways in which it is



more often effectively practiced—bring us back to questions of method. To have dealt with that more directly might have embarrassingly shortened this essay: there is no comparative method in history.<sup>29</sup> “The comparative method” is a phrase as redolent of the nineteenth century as “the historical method” with which it was once nearly synonymous. For historians, at least, the idea of comparison still needs to be demystified. Abstract discussion of historical comparison makes it seem so demanding in terms of knowledge and method and so taxing and complicated a procedure that only the most immodest could feel capable of attempting it and all of the rest are excused from trying. Indeed, scholars reluctant to break the framework of received opinions have an interest in adopting that view. But historical comparison is no more attached to a single method than is the discipline of history itself.

John Stuart Mill’s oft-cited distinction between the Method of Agreement and the Method of Difference is a contribution to logic, but it leaves the way open to different kinds of comparisons according to whether one is exploring new questions, defining a problem, isolating a single variable, identifying general patterns, or testing a hypothesis. The criteria for selecting which elements to compare, for checking the internal logic of the analysis, or for determining the relevance of the evidence used are not fundamentally different for comparison than for social analysis of any sort. There are not even general rules, except those of logic, and they apply differently according to one’s purpose, as to whether it is better to compare cases that are similar or contrasting, neighboring or distant, synchronic or diachronic.<sup>30</sup>

To call for comparison is to call for a kind of attitude—open, questioning, searching—and to suggest some practices that may nourish it, to ask historians to think in terms of problems and dare to define those problems independently, and to assert that even the narrowest research should be conceived in terms of the larger quests of many scholars in many fields. To call for comparison, however, says almost nothing about how to do any of this well.<sup>31</sup> In the United States, the excitement of comparison may be found more often in classrooms than in print and in the colleges where a few teach broadly than in the universities where many research narrowly. It may even be that this use of comparison is stimulating some of the venturesome work of younger scholars.

<sup>29</sup> “So the methodological problems facing ‘comparativists’ are the same as those facing all social-scientific investigators”; Neil J. Smelser, *Comparative Methods in the Social Sciences* (Englewood Cliffs, N.J., 1976), 3. Historians will find the discussions of method useful in Smelser’s book and in Stinchcombe’s *Theoretical Methods in Social History* (and especially enjoy comparing the two discussions of comparison in de Tocqueville) as well as in Vallier’s *Comparative Methods in Sociology* and Etzioni and Dubow’s *Comparative Perspectives: Theories and Methods*.

<sup>30</sup> Arend Lijphart’s valiant effort to define a comparative methodology for political science (a discipline in which it might be expected that comparison would be more readily controlled than it is in history) does not seem to me to be convincing; see Lijphart, “Comparative Politics and the Comparative Method,” *American Political Science Review*, 65 (1971): 682–93, and “Comparative Research.” Also see Theodore W. Meckstroth, “‘Most Different Systems’ and ‘Most Similar Systems,’” *Comparative Political Studies*, 8 (July 1975): 132–57; and Henry Teune, “Comparative Research, Experimental Design, and the Comparative Method,” *ibid.*, 195–99. On the response to Mill by Émile Durkheim and Max Weber, see Smelser, *Comparative Methods in the Social Sciences*, 62–67, 141–43.

<sup>31</sup> “The choice of the phenomena to be explained is the responsibility of the historian, not the comparative method”; William H. Sewell, Jr., “Marc Bloch and the Logic of Comparative History,” *History and Theory*, 6 (1967): 213; also see *ibid.*, 213–18.



To admit that comparison contains no special method is not to say that methodology is unimportant. The search for problems is more than blind intuition and will benefit from the most formal methods applicable. Nor is the search for patterns mad empiricism, for it should welcome models and hypotheses that are as well developed as possible. Methods behavioral, quantitative, inductive, and even deductive, formal models, and theories of change can all be applied to comparative study and used to guide the historian in determining when comparisons support generalization and when generalization can be stretched to theory. Even the historian's concern for whether a hypothesis really fits a given context can be a disciplined test.<sup>32</sup>

Not only is comparison not a method, but "comparative history" is a term better avoided. In his famous call for comparison, Marc Bloch warned he would put forth no "new panacea," and it is worth noting that he spoke of *histoire comparée*, not *histoire comparative*. For historians to think comparatively, to compare histories, is to do what we already do—a little more consciously and on a somewhat broader plane. It is not to embrace some new type or genre of historical investigation. Bloch cited Fustel de Coulange's aphorism that a day of synthesis requires years of analysis, quickly adding that "analysis can be used for synthetic purposes only if it intends from the outset to contribute to such a synthesis."<sup>33</sup> That warning points to the essence of historical comparison—the conduct of research and the presentation of findings in the context of a larger continuing discourse about the nature of society.

There is thus no paradox in insisting that the study of a single case can be comparative.<sup>34</sup> The use of common terms and recognizable categories assumes comparison. The search for patterns implies comparison by seeking regularities in the behavior and issues common to a certain set—a kind of group, institution, or form of social organization—or common to a certain process.<sup>35</sup> In searching for patterns the historian is following François Simiand's injunction of seventy-five years ago to study classes of problems. Elegant solutions to problems carefully posed invite further comparisons, the testing of patterns and hypotheses that is most generally recognized as a form of experiment.<sup>36</sup> At its best, the practice of comparison can lead toward that *histoire générale* for which Lucien Febvre argued and also establish agenda for concrete, new research. At its least, comparison is an effective device for removing the inexcusable blinders of parochialism.

<sup>32</sup> See Donald T. Campbell's article, all the more impressive for its revision of his earlier argument, "'Degrees of Freedom' and 'The Case Study,'" *Comparative Political Studies*, 8 (July 1975): 178–93.

<sup>33</sup> Bloch, "Pour une histoire comparée," 16, 38. Presumably, the analogue of the term was *grammaire comparée*.

<sup>34</sup> William H. Flanigan and Nancy H. Zingale made this point in "The Comparative Method in Political Science," paper delivered at the annual meeting of the American Historical Association held in San Francisco, December 1978; so did E. A. Hammel in "The Comparative Method in Anthropological Perspective," a paper at the same session. An earlier version of this essay completed the panel at that meeting.

<sup>35</sup> Alexander Gerschenkron has called the industrial history of Europe "a unified and yet graduated pattern"; Gerschenkron, *Economic Backwardness in Historical Perspective* (Cambridge, Mass., 1966), 1.

<sup>36</sup> And these can be used to test "presumptive causal linkages"; Walter P. Metzger, "Generalizations about National Character: An Analytic Essay," in Louis Gottschalk, ed., *Generalization in the Writing of History* (Chicago, 1963), 90.

Articles and books need not do all of this explicitly, and, happily, it is impossible to list all possible comparisons relevant to any single case. But authors and readers who think in comparative terms enlarge the communication on which science rests. Even their tacit comparison eases the task of integrating concepts and methods from different disciplines by providing common categories of analysis and a common focus, the most effective counterweight to the penalties of specialization. Within the framework of comparison a dialogue and even a kind of teamwork can then cut across our disciplinary divisions. Arthur Wright once said that for the Chinese the discovery of comparative history and comparative sociology “was revolutionary in its effects on historical studies.”<sup>37</sup> We would do well to work to make that revolution permanent in its historical home.

<sup>37</sup> Arthur F. Wright, “On the Uses of Generalization in the Study of Chinese History,” in Gottschalk, *Generalization in the Writing of History*, 46.

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## Antebellum Planters and *Vormärz* Junkers in Comparative Perspective

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SHEARER DAVIS BOWMAN

EVER SINCE PLANTATION MAGNATES IN COLONIAL VIRGINIA began to think of themselves as transplanted English country gentlemen, the friends, foes, and scholarly interpreters of the South's slaveholding planters have, on occasion, suggested their comparability to Europe's traditional feudal aristocracies. In view of the longstanding scholarly debate over the character of the Old South's planter class and proslavery ideology, historians have been surprisingly slow to make systematic comparisons of antebellum planters and Europe's surviving landed aristocracies of the same epoch. Although Peter Kolchin has focused on the *pomeshchiki* estate owners and the proserfdom thought of pre-1861 Russia as a standard of comparison,<sup>1</sup> the Junker gentry of eastern Prussia are frequently cited as the most appropriate historical analogue to the planters of the Old South.<sup>2</sup> The Junkers were certainly the most formidable landed elite in nineteenth-century Europe, and, in "Old Prussian" ideology, their apologists presented Europe's most intellectually rigorous and politically effective "aristocratic" opposition to the "French ideas of 1789."

By comparing antebellum planters with pre-1848 (*Vormärz*) Junkers, I hope to contribute a new perspective to the ongoing controversy over whether the planters were a genuinely "conservative" class and whether Southern proslavery

This essay is a revised and expanded version of a paper presented at the Ninety-Third Annual Meeting of the American Historical Association, held in San Francisco, December 28, 1978. For their valuable criticisms of the AHA paper or for helpful suggestions at various times during my research and writing, I am indebted to Kenneth Stampp, Gerald Feldman, Reinhard Bendix, Hans Rosenberg, George Fredrickson, Michael Johnson, and Rudolf von Thadden, to my fellow Berkeley graduate students Robert Moeller, William McNeil, James Oakes, James Gregory, William Gienapp, and Cornelius Gispert, to Dr. Josef Mooser of Bielefeld, and to four anonymous referees for the *American Historical Review*. Of course, I alone am responsible for any flaws and deficiencies in the final product.

<sup>1</sup> Kolchin, "In Defense of Servitude: American Proslavery and Russian Proserfdom Arguments, 1760-1860," *AHR*, pages 809-27, below, originally presented as a paper at the Ninety-Third Annual Meeting of the American Historical Association, held in San Francisco, December 28, 1978.

<sup>2</sup> See, in particular, Dietrich Zwickler, *Der amerikanische Staatsmann John C. Calhoun, Ein Kämpfer gegen die "Ideen von 1789": Studien zur Vorgeschichte des amerikanischen Bürgerkrieges*, Historische Studien, no. 280 (Berlin, 1935), pt. 2, chap. 1: "Gleichzeitige Ideen in Deutschland?" esp. 110-12; and the tantalizing comments in Barrington Moore, Jr., *Social Origins of Dictatorship and Democracy: Lord and Peasant in the Making of the Modern World* (Boston, 1966), chap. 3: "The American Civil War: The Last Capitalist Revolution," esp. 115, 127. For pointed criticism of Moore's handling of the comparison, see Eugene D. Genovese, "Marxian Interpretations of the Slave South," in Barton Bernstein, ed., *Towards a New Past: Dissenting Essays in American History* (New York, 1968), 117-20, and *The World the Slaveholders Made: Two Essays in Interpretation* (New York, 1969), 228-30.

thought was a genuinely “conservative” ideology. The starkly contrasting views of two prominent American scholars illustrate the nature of the controversy. Louis Hartz has belittled the conservative authenticity of the planters and of proslavery thought by stressing America’s lack of a feudal heritage, the South’s inescapable attachment to the American liberal tradition (its “prejudices of liberty and equality”), and the middle-class, capitalist character of Southern society.<sup>3</sup> Eugene D. Genovese, however, has argued for the genuine conservatism of the planters and of proslavery thought by insisting upon the “precapitalist” character of the Old South’s “paternalistic” master-slave relationship and the consequent “prebourgeois” outlook of antebellum planters—“the closest thing to feudal lords imaginable in a nineteenth-century bourgeois republic.”<sup>4</sup> Hartz and Genovese seem to agree, however, that the planters could not simultaneously be both capitalists and authentic conservatives, a judgment that this essay will question.

Before addressing the problematical concept of conservatism and its applicability to planters and Junkers, it is necessary to establish some structural and attitudinal similarities between the two landed elites and some fundamental dissimilarities between their historical environments.<sup>5</sup> My analysis of Southern planters concentrates on the four antebellum decades from the Missouri controversy of 1819–21 over slavery’s westward expansion until secession and the outbreak of the Civil War in 1861. In discussing the Junkers, my focus is on the period from Baron vom Stein’s edict abolishing serfdom in 1807 to the revolutions of 1848–49, which brought about the final liquidation of surviving servile dues. My essay will conclude with a comparison of the attitudes and behavior of planters and Junkers during the political crises of 1860–61 and 1848, since the particular “conservative” qualities of each landed elite manifested themselves with special clarity at these times of intensified stress and conflict.

JUNKERS AND PLANTERS CAN BE TERMED “gentlemen farmers,” though not in the sense of English landlords living off tenant rents. They were gentlemen farmers in that they were freed not only from the necessity of manual labor but also from the need to serve as full-time supervisors of work in the fields.<sup>6</sup> The term

<sup>3</sup> Hartz, *The Liberal Tradition in America: An Interpretation of American Political Thought since the Revolution* (New York, 1955), pt. 4: “The Feudal Dream of the South,” esp. 146–54, 189–97.

<sup>4</sup> Genovese, *The Political Economy of Slavery: Studies in the Economy and Society of the Slave South* (New York, 1967), chap. 1: “The Slave South: An Interpretation,” esp. 31. Genovese has developed this interpretation further; see his *The World the Slaveholders Made*, esp. 96–99, 121–26, and *Roll, Jordan, Roll: The World the Slaves Made* (New York, 1974), esp. 3–7, 661–63.

<sup>5</sup> As Marc Bloch pointed out in 1928, “In order to have historical comparison, two conditions must be fulfilled: a certain similarity or analogy between observed phenomena—that is obvious—and a certain dissimilarity between the environments in which they occur”; Bloch, “Toward a Comparative History of European Societies,” trans. Jelle C. Riemersma, in Frederic C. Lane and Jelle C. Riemersma, eds., *Enterprise and Secular Change: Readings in Economic History* (Homewood, Ill., 1953), 496.

<sup>6</sup> As Peter Laslett has explained, “The primary characteristic of the gentleman was that he never worked with his hands on necessary, as opposed to leisurely, activities”; Laslett, *The World We Have Lost: England before the Industrial Age* (1966; 2d ed., New York, 1971), 30. To inject the English term “gentleman” into a discussion of Prussian history is admittedly problematic, especially since the German language has no precise equivalent.

“Junker” (derived from the Middle High German for “young nobleman”) identifies both noble and nonnoble owners of legally privileged estates (*Rittergüter*) in Prussia’s six eastern provinces. Except for part of Brandenburg and most of Saxony, these provinces lay to the east of the Elbe River and are, therefore, referred to collectively as “East Elbia.” Although the eleven thousand five hundred *Rittergüter* (literally, “knights’ estates”) in East Elbia varied in size even more than Southern plantations, the majority were larger than the 379 acres (600 *Morgen*) specified by government statisticians as the minimum for a “large landed estate.” In 1851 the average *Rittergut* encompassed between 1,300 and 1,400 acres, had perhaps twenty to twenty-five resident families providing full-time agricultural labor, and employed a manager and often field foremen as well.<sup>7</sup>

The term “planter” denotes the larger and more elastic elite of agricultural slaveowners whose holdings in land and slaves were large enough that the job of superintending labor in the fields usually fell to white overseers or black foremen (drivers). This imprecise definition includes some slaveholders with fewer than the twenty slaves specified as the minimum for classification as a “planter” by census officials in 1860, who counted forty-six thousand planters; but the imprecision reflects the social fluidity of the antebellum planter class more accurately than does the census-takers’ standard.<sup>8</sup> One historian has suggested that the “accurate definition of a planter” should also include ownership of “a minimum of between 500 and 1,000 acres.”<sup>9</sup> Here, again, a more flexible standard is necessary, even to accommodate some substantial slaveholders like Charles Whitmore of Mississippi, who owned about thirty-five slaves and just over 300 acres near Natchez during the 1840s and 1850s, and Robert Christian of the Alabama

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My purpose is simply to emphasize that freedom from the necessity of arduous physical labor and the concomitant freedom to enjoy civilized leisure were valued in the rural societies of both the South and eastern Prussia as prerequisites to social pre-eminence and, therefore, to popular recognition of elite status.

<sup>7</sup> In 1851, over eleven thousand five hundred estates were classified as *Rittergüter* in East Elbia. I computed the number of estates and their average acreage from statistics in C. F. W. Dieterici, *Handbuch der Statistik des preussischen Staats* (Berlin, 1861), 318. The largest *Rittergut* encompassed 46,002 acres (72,904 *Morgen*; one *Morgen* equaled 0.631 acres) and was located in the province of Prussia, while the smallest *Rittergut*, located in Silesia, encompassed about six-tenths of an acre. As Dieterici, a government statistician, explained, “The status of a *Rittergut* is independent of the estate’s size; it is quite possible for a *Rittergut* owner to sell arable land and fields and to retain for the house and small estate remaining the status of a *Rittergut*”; *ibid.*, 320–21.

<sup>8</sup> Planters with more than thirty slaves tended to have the most efficient and complex operations and generally had both overseers and drivers, while planters who owned fewer than thirty slaves tended to rely on their sons or slave foremen. “Masters who had at their command as few as half a dozen field-hands were tempted to improve their social status by withdrawing from the fields and devoting most of their time to managerial functions.” Kenneth M. Stampp, *The Peculiar Institution: Slavery in the Antebellum South* (New York, 1956), 35–38. One such small-scale but ambitious cotton planter was James Monette, of Morehouse Parish in northern Louisiana. During the fall of 1853 Monette had six to eight male and female pickers working in the fields, and his crop for that year amounted to a very respectable fifty bales, or 22,305 pounds. James Monette Day Book and Diary, 1848–1863 (typescript), Library of Congress. The most thorough student of antebellum agriculture, Lewis Cecil Gray, has distinguished between upper-class planters who owned fifty or more slaves and the much larger group of middle-class planters who owned ten to fifty slaves. Thus, according to Gray, there were nearly one hundred and ten thousand “planters” in 1860, out of a total of three hundred and eighty-five thousand slaveowners. Gray, *History of Agriculture in the Southern United States to 1860*, 1 (1933; reprint ed., New York, 1958), 481–83. My own definition of planters would place the number between Gray’s figure and the census figure of 1860, but definitely closer to the latter. Although planters composed a small minority of slaveholders as well as of the South’s white population, they owned well over half of the almost four million slaves in 1860.

<sup>9</sup> Clement Eaton, *A History of the Old South* (3d ed., New York, 1975), 390.

Black Belt, who had twenty-five slaves and 400 acres in 1857.<sup>10</sup> The planter class was a more fluid elite than the Junkers, both geographically and socially, as the increase in the number of "Southern" slave states from eleven to fifteen during the antebellum decades demonstrates. Of course, intraregional variations existed among planters and plantations in the South, just as they did among Junkers and *Rittergüter* in East Elbia. Not only geography but also notable economic and cultural differences separated Virginia and Louisiana, for example, or Pomerania and Silesia. But plantations throughout the South, like *Rittergüter* throughout East Elbia, displayed certain fundamental similarities as social institutions that transcended intraregional distinctions.

Although the legal and racial status of slaves on a plantation was certainly quite different from that of the laborers on a Junker estate (before as well as after the end of hereditary bondage in 1807), there were significant parallels between the productive purposes to which menials on plantations and *Rittergüter* were put and between the ways in which they were governed. Each work force was subject to the personal, nearly despotic, authority of the owner, and each worked to produce cash crops for foreign and domestic markets. While Southern planters were growing cotton or tobacco for shipment to Liverpool or New York, for example, East Elbian Junkers were producing wheat or wool for shipment to London or Berlin. At mid-century most plantations and *Rittergüter* also achieved a high, cost-efficient level of self-sufficiency in basic foodstuffs as well. The functional and structural analogies between the plantation and the *Rittergut* are crucial to a comparative study of planters and Junkers, because these estates and their work forces constituted the foundations of their owners' wealth, political influence, social status, and, in many instances, even their self-esteem.<sup>11</sup>

During the first half of the nineteenth century, moreover, the owners of *Rittergüter* became what the owners of plantations had always been—a socially mixed class of agrarian entrepreneurs. After Stein's edict of October 1807 declared an end to both serfdom and the nobility's de jure monopoly on the ownership of Junker estates, any wealthy and ambitious "commoner" could legally purchase a *Rittergut* and the aristocratic privileges tied to it. By 1856, 45 percent belonged to untitled commoners, including one who was the son of a rural cottager.<sup>12</sup> But the purchase of Junker estates by many nonnobles is not as signifi-

<sup>10</sup> Mark Swearingen, "Thirty Years of a Mississippi Plantation: Charles Whitmore of 'Montpelier,'" *Journal of Southern History*, 1 (1935): 201; and the Robert Christian Diary, 1854–57, "Fair Oaks," Perry County, Alabama, Alabama State Department of Archives and History, Montgomery, Ala. Both Whitmore and Christian were cotton planters.

<sup>11</sup> For the importance of his land and work force to a planter's public stature and self-esteem, see James L. Roark, *Masters without Slaves: Southern Planters in the Civil War and Reconstruction* (New York, 1977), esp. 35. For a comparable perspective on the Junkers, see Heinrich Heffter, *Die deutsche Selbstverwaltung im 19. Jahrhundert: Geschichte der Ideen und Institutionen* (Stuttgart, 1950), 132.

<sup>12</sup> I have computed the percentage of *Rittergüter* owned by commoners from figures in Karl Friedrich Rauer, *Hand-Matrikel der in sämtlichen Kreisen des preussischen Staats auf Kreis- und Landtagen vertretenen Rittergüter* (Berlin, 1857), 451. The cottager's son was Johann Gottlieb Koppe, a widely respected agronomist who worked his way up first as the efficient manager of a noble-owned *Rittergut* and then as the innovative lessee of a crown estate. In 1842 he bought two *Rittergüter* in Brandenburg, and shortly thereafter he was appointed to the *Landes-Oeconomie-Collegium*, a fifteen-member committee under the Ministry of the Interior and the most prestigious governmental agency for agricultural affairs in Prussia. See "Johann Gottlieb Koppe," *Allgemeine Deutsche Biographie*, 16 (Leipzig, 1882): 693–97.



cant as the influence that the free trade in these estates had on the character of the Junkers as a landed elite, especially during the depression years of the 1820s. Titled Junkers who held on to their estates were usually able to do so only because they adopted the innovative, cost-conscious spirit of the nonnoble newcomers to their ranks.<sup>13</sup> Captain Carl von Wulffen-Pietzpuhl of Brandenburg, writing in 1845 about the educational value of model farms for “the advancement of Prussia’s practical agriculture,” declared that “the most rational” farmer managed to use “land and soil most effectively” and that “the most important aspect of rational agriculture” could be “reduced to the art of producing the cheapest dung.”<sup>14</sup> Mid-century Junkers, like antebellum planters, understood that a profitable commercial agriculture was essential in the long run to their survival as landed gentlemen. Both elites can be evaluated as “agrarian capitalists” in that they were impelled by the need and desire to earn substantial profits from their capital investments in producing agricultural commodities.<sup>15</sup>

Describing antebellum planters and pre-1848 (*Vormärz*) Junkers as agrarian capitalists, while also recognizing that they generally valued the status and lifestyle of a landed gentleman as ends in themselves, does not entail any inconsistency or contradiction. Alfred Huger of South Carolina complained about “the fools and swindlers” on New York’s Wall Street and described himself as “a ‘Plantation Man’ ” who treasured “the quiet and retirement of Plantation Life.” Yet in 1856 he offered sound advice about establishing a successful farm to the son of an old friend off to settle in turbulent Kansas: look for “good land selected where a town is most likely to be established, and the facility of reaching the point where crops are to be sold”; in particular, “look always to a water course—the shortest way to market is the shortest way to Wealth.”<sup>16</sup>

<sup>13</sup> For the most compelling presentation of this argument, see Hans Rosenberg, “Die Pseudodemokratisierung der Rittergutsbesitzerklasse” (1958), in his *Probleme der deutschen Sozialgeschichte* (Frankfurt a/M., 1969), 14, 17, 21–22. For British historian David Spring’s recent misinterpretation of Rosenberg’s argument, see Spring, “Landed Elites Compared,” in Spring, ed., *European Landed Elites in the Nineteenth Century* (Baltimore, 1977), 5, 19 n. 20. And, for reaffirmations of Rosenberg’s interpretation, see Reinhart Koselleck, *Preussen zwischen Reform und Revolution: Allgemeines Landrecht, Verwaltung, und soziale Bewegung von 1791 bis 1848* (Stuttgart, 1967), 512–14; and Hanna Schissler, *Preussische Agrargesellschaft im Wandel: Wirtschaftliche, gesellschaftliche, und politische Transformationsprozesse von 1763 bis 1847* (Göttingen, 1978), 165–67, 199–201.

<sup>14</sup> Hauptmann von Wulffen auf Pietzpuhl, “Gutachten betreffend die von Herrn Franz zu Eggenstadt bei Seehausen . . . mitgetheilte Denkschrift: Über die Notwendigkeit eigenthümlich gestellter Musterwirtschaften behufs der Förderung der praktischen Landwirtschaft Preussens,” *Annalen der Landwirtschaft in der königl. Preussischen Staaten*, 5 (1845): 60.

<sup>15</sup> In his April 1979 presidential address to the Organization of American Historians, Eugene D. Genovese reiterated his argument that the slave South had a “precapitalist” economic system, that “the liberation of entrepreneurship historically accompanied the free market, especially in labor power, and [that] entrepreneurship, like science, technology, education, and investment in ‘human capital’ in general, arose as a function of freedom and everywhere suffered in the absence of freedom”; Genovese and Elizabeth Fox-Genovese, “The Slave Economies in Political Perspective,” *Journal of American History*, 66 (1979): 22, 15. This perspective seems to confuse the historical conditions prerequisite to the development and flowering of “industrial capitalism” with the entrepreneurial, market- and profit-oriented behavior common to all capitalists. I agree with Jürgen Kocka’s definition of capitalism as “an economic system that rests predominantly on private property and private disposition over capital and that facilitates production and exchange of commodities for the purpose of profit.” As Kocka has suggested, legally free workers under contract and receiving wages are a defining characteristic of “modern industrial capitalism.” Kocka, *Klassengesellschaft im Krieg: Deutsche Sozialgeschichte, 1914–1918* (Göttingen, 1973), 149 n. 14.

<sup>16</sup> Huger to R. Bunch, October 20, 1857, to William Porcher Miles, January 23, 1958, and to John Vanderhorst, August 1, 1856; Alfred Huger Letterpress Books, 1853–1863, Perkins Library, Duke University, Durham, N.C.

Although landed Junkerdom was a more fluid and entrepreneurial class in 1850 than in 1800, even on the eve of Stein's edict of 1807 the Junkers and their serfs were no more a historical legacy of noncommercial seigneurialism than were Southern planters in the era of Jefferson and Madison. *Rittergut* owners in regions bordering the Baltic Sea used serf labor to produce grain for export to Western and Southern Europe a century and a half before the founders of colonial Virginia's plantation gentry began shipping tobacco to England; and serfdom east of the Elbe River only became established in the late fifteenth to the mid-seventeenth centuries. Like plantation slavery in the colonial South, East Elbian serfdom evolved primarily as a combined entrepreneurial and political maneuver to secure a labor force suitable for the production of cash crops on large estates. As German historian Georg Friedrich Knapp observed in 1891, "The modern big business concern of the *Rittergut* began with the forced labor of unfree persons, just as the modern plantation enterprise began with the forced labor of the unfree; but the planter obtained Negroes in Africa and made them slaves; the *Rittergut* owner did not reach so far, for he seized his peasants and made them serfs."<sup>17</sup> Thus, both Junkers and planters established themselves as landed elites long before the onset of the Industrial Revolution, during the "early modern" centuries that witnessed the emergence of what Immanuel Wallerstein has termed a "capitalist world-economy" centered in Britain and the Low Countries.<sup>18</sup> But the planters' and Junkers' fortunes as agricultural entrepreneurs were revitalized during the late eighteenth and early nineteenth centuries by the rise of English industrial capitalism and the growth of English demand for foreign wheat, cotton, and wool. In East Elbia this revitalization of large-scale, cash-crop agriculture involved the abolition of serfdom, while in the American South it brought the expansion of slavery.

During the three *Vormärz* decades prior to the Revolution of 1848, the make-up of the Junkers' labor force and organization of production on their estates underwent far-reaching transformations that could have had no parallel in the very different historical context of the Old South. The Stein-Hardenberg agrarian reforms, instituted piecemeal by the Prussian bureaucracy between 1807 and 1821, compelled some inveterately traditionalist Junkers—and allowed others more flexible in their attitudes—to make a gradual transition from serf labor to a contractual work force. These agrarian reforms entitled the *Rittergut* owner to demand from his freed and "regulated" (*reguliert*) peasants, as compensation for the abolition of serfdom and compulsory labor, long-term commutation payments or the cession of peasant land. Under Hardenberg's retrogressive Declaration of 1816, however, the majority of former serfs were made ineligible for the regulation of their servile obligations, because either the size of their landholdings or the longevity of their titles to the land did not meet the legal requirements. Very often these unregulated peasants, already suffering from the

<sup>17</sup> Knapp, "Die Erbuntertänigkeit und die kapitalistische Wirtschaft," in his *Die Landarbeiter in Knechtschaft und Freiheit* (2d ed., Leipzig, 1909), 57.

<sup>18</sup> Wallerstein, *The Modern World-System: Capitalist Agriculture and the Origins of the European World Economy in the Sixteenth Century* (New York, 1974), 88–91, 99–100.

loss of their former rights to use the common lands, found it impossible to support themselves on their own small farms while having to pay state taxes and perform services for their former lords. Consequently, they sold out to the *Rittergut* owner but usually continued to work for him as cottagers on annual contracts (*Insten* and *Lohngärtner*).

These cottagers became the most important group of workers on Junker estates during the middle decades of the nineteenth century, although Junkers also employed contractual servants (*Gesinde*) and often hired extra day-laborers for the harvest or major agricultural improvements (just as some Southern planters hired free laborers to undertake projects like ditching and secured outside free or slave labor at harvest time). The East Elbian cottager and his family generally received a small house on a half acre of garden land suitable for potatoes and vegetables and the right to graze a few animals on the lord's meadow—the sort of compensation, one might say, that many slaves also received from their masters. But the cottager family received, in addition, the yield from several acres of the Junker's crop land, a fraction of the Junker's grain threshed by the cottager during the fall and winter months, and a small monetary wage. In return, two and sometimes three members of the cottager's family were required to work year-round on the *Rittergut* for six days per week (minus occasional vacations) and from sunrise to sunset.<sup>19</sup>

The Stein-Hardenberg reforms, by enabling the Junker to consolidate his heretofore scattered fields and to absorb former peasant and common land into his own estate, both facilitated and spurred the Junkers' transition to more productive, complex, and labor-intensive systems of crop rotation and soil fertilization adapted from English practices. The rapid growth of East Elbia's population during *Vormärz* provided the labor necessary to bring new land under the plow and to cultivate new field crops (especially root crops) at a time when agrarian technology was not yet able to substitute mechanical for human and animal power on a large scale.<sup>20</sup> In the antebellum South, human labor was never so cheap or plentiful as in prerevolutionary Prussia, despite the increase in the slave population from one and a half million in 1820 to nearly four million in 1860. But Negro slavery enabled the planters to avoid the labor shortage that confronted Northern farmers and, at the same time, increased the amount of money that could be profitably invested in plantation agriculture.<sup>21</sup> Moreover,

<sup>19</sup> The following works are especially informative on cottagers: Oberamtmann Proseleger, "Über den Zustand der landwirtschaftlichen Verhältnisse in Graudenzer Kreis, 1843," *Annalen der Landwirtschaft*, 8 (1846): 74–78, reprinted in Georg Friedrich Knapp, *Die Bauernbefreiung und die Ursprung der Landarbeiter in den älteren Theilen Preussens*, 1 (2d ed., Munich, 1927): 337–40; Max Weber, *Die Verhältnisse der Landarbeiter im ostelbischen Deutschland*, vol. 55 of *Schriften des Vereins für Sozialpolitik* (Leipzig, 1892), 11–18; and Günther Franz, "Landwirtschaft, 1800–1850," in Hermann Aubin and Wolfgang Zorn, eds., *Handbuch der deutschen Wirtschafts- und Sozialgeschichte*, 2 (Stuttgart, 1976): 302–04.

<sup>20</sup> "The immense increase in Prussia's population from roughly ten to sixteen million between 1815 and 1848 came mostly from the rural districts of the East Elbian provinces, where the agrarian reforms made possible, and at the same time encouraged, that growth"; Koselleck, *Preussen zwischen Reform und Revolution*, 503. As Günther Franz has pointed out, human and animal power predominated in East Elbia at least until the post-1850 development of the steam plow; "Landwirtschaft, 1800–1850," 292.

<sup>21</sup> Heywood Fleisig has suggested that slavery, by relaxing the labor constraint that Southern planters would have faced under a free-labor system, may have served to retard industrialization in the South by in-

slavery, unlike serfdom, had never presented obstacles to accelerated innovation and profitability in Southern agriculture, for slaves had neither formal ties to nor claims upon the land and, therefore, presented no hindrances to the planter's free disposition over his property or to a "rational" concentration of land in fewer hands.

If antebellum planters were, on the whole, slower than *Vormärz* Junkers to institute improved crop rotations, stall feeding of livestock, and manuring or marling, the fundamental explanation is threefold: in the South fertile land was cheaper and more abundant, fewer mouths had to be fed from the same plot of land because population density was much lower,<sup>22</sup> and plantation labor was scarcer and dearer, which encouraged most planters to concentrate their efforts on immediately profitable staples rather than on diversified agriculture. Yet slavery was well suited to the plantation economy's need for a supply of gang laborers whose geographical mobility made them highly responsive to constant, often rapid, shifts in demand along an advancing frontier.<sup>23</sup> Furthermore, slaves, as chattel, were themselves a lucrative capital investment during the antebellum decades. Hence, Thomas Affleck's widely used *Cotton Plantation Record and Account Book* included a section on "The Duties of an Overseer," which stipulated that the primary component of "a *fine crop*" was "an increase in the number and a marked increase in the condition and value of the negroes."<sup>24</sup>

BOTH PLANTATION and *Rittergut* had a commercial-political character—a cash-crop business combined with a semi-autonomous community over which a single individual wielded inheritable and salable patrimonial power. In other words, plantation and *Rittergut* constituted what an antebellum Virginia academic termed "an *imperium in imperio*—a government within a government" or what a German historian has more recently called "a formidable 'private law state.'"<sup>25</sup> The nearly despotic authority that an antebellum planter or *Vormärz*

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creasing the amount of money that could profitably be invested in farming; Fleisig, "Slavery, the Supply of Agricultural Labor, and the Industrialization of the South," *Journal of Economic History*, 36 (1976): 581–83. Hanna Schissler made a comparable argument for East Elbia, claiming that agricultural growth during *Vormärz* benefited from population growth but hindered industrialization "because agriculture itself absorbed labor power, attracted entrepreneurial talent, and needed capital"; *Preussische Agrargesellschaft im Wandel*, 187.

<sup>22</sup> The population per square mile in the coastal planting states (South Carolina, Georgia, Florida, Alabama, Mississippi, and Louisiana) was 15.25 in 1860 and 20.93 for the central slave states (Virginia, North Carolina, Kentucky, Tennessee, Missouri, and Arkansas) in the same year; U.S. Census Office, *Preliminary Report of the Eighth Census, 1860* (Washington, 1862), 8. In 1858 the Baltic provinces of Prussia, Posen, and Pomerania in East Elbia—the least populous by far of Prussia's eight provinces—contained approximately 113 persons per square mile; I have computed this figure from statistics in Georg von Viebahn, *Statistik des zollvereinten und nördlichen Deutschlands*, 2 (Berlin, 1862): 164.

<sup>23</sup> Edgar T. Thompson, "The Plantation: The Physical Basis of Traditional Race Relations" (1939), in his *Plantation Societies, Race Relations, and the South—The Regimentation of Populations: Selected Papers of Edgar T. Thompson* (Durham, N.C., 1975), 89–90.

<sup>24</sup> Affleck, "The Duties of an Overseer," reprinted in the *American Cotton Planter*, 2 (1854): 356.

<sup>25</sup> William A. Smith (President of Randolph Macon College), *Lectures on the Philosophy and Practice of Slavery* (Nashville, Tenn., 1856), 154; and Hans Rosenberg, *Bureaucracy, Aristocracy, and Autocracy: The Prussian Experience, 1660–1815* (Boston, 1958), 43.

Junker exercised over those who lived and labored on his land could and did serve as an invaluable instrument of direct economic coercion;<sup>26</sup> but in the minds of most planters and Junkers maintaining the subordinate position of their laborers assumed supra-economic significance as a social, political, or—in the case of the South—racial necessity as well. To be sure, the legal source of a planter's authority over his slaves was not the equivalent of a Junker's over his cottagers and servants. A planter's autocratic power derived from his ownership of slaves as personal property, as chattel, not from his ownership of any particular tract of land on which the slaves worked. By contrast, the authority of a Junker, whether nobleman or commoner, derived from his ownership of the *Rittergut*, which entitled him to exercise, either in person or through a personally designated representative, such powers of local government as police authority, lesser justice, and supervision over church and school affairs. Thus the Junker was a *Gutsherr*, a lord of an estate, while the planter was a master of slaves.<sup>27</sup>

Idealistic, romantic spokesmen for the two landed elites tended to portray government on both plantation and *Rittergut* as a familial, patriarchal relationship between "benevolent fathers" and "obedient sons."<sup>28</sup> James H. Hammond of South Carolina, in a pair of widely quoted letters written in 1845 to the president of the British Anti-Slavery Society, described antebellum plantation practices as "our patriarchal scheme of domestic servitude" and maintained that few social ties "are more heartfelt, or of more beneficent influence, than those which mutually bind the master and the slave."<sup>29</sup> F. A. Ludwig von der Marwitz of Brandenburg argued in 1811, in response to Stein's condemnation of serfdom as "the last vestige of slavery," that "serfdom was rather a patriarchal bond that joined the peasants to the nobleman" and that the *Rittergut* constituted a "small patriarchal state" governed by "a member of the family."<sup>30</sup> A few iconoclastic planters and Junkers, however, vigorously decried such apologetic incantations of paternalism. East Elbia's closest equivalent to the South's James G. Birney, the Alabama lawyer-planter who became a vocal abolitionist in the 1830s, was probably the Silesian Count zu Dohna-Kotzenau, whose strictures of the status quo won him the title "the red count." In a short book published in 1847, the purpose of which was to stir up public concern about the deplorable conditions

<sup>26</sup> The issue is clear-cut regarding the planters. On the Junkers, see Koselleck, *Preussen zwischen Reform und Revolution*, 539–53; and Schissler, *Preussische Agrargesellschaft im Wandel*, 167–77.

<sup>27</sup> The Junker's *Gutsherrschaft*—literally, the authority of an estate—extended over the inhabitants of the small rural district (*Gutsbezirk*) within which his own *Rittergut* was located. After the Stein-Hardenberg reforms the Junker's authority became less significant for the peasant villages outside the boundaries of his own estate and more important for the cottagers and servants who lived and labored on the *Rittergut*. Koselleck, *Preussen zwischen Reform und Revolution*, 544–49. The Junker's patrimonial judicial authority was abolished in 1849, but his far more exploitable right to exercise local police power survived until 1872.

<sup>28</sup> Robert M. Bigler, *The Politics of German Protestantism: The Rise of the Protestant Church Elite in Prussia, 1815–1848* (Berkeley and Los Angeles, 1972), 147. Stampf observed of slavery that "ideally it was the relationship of parent and child"; *The Peculiar Institution*, 327.

<sup>29</sup> Hammond, "Two Letters on the Subject of Slavery in the United States, Addressed to Thomas Clarkson, Esq.," in Hammond, *Selections from the Letters and Speeches of the Hon. James H. Hammond* (New York, 1866), 184. These letters were published before the Civil War under the title "Slavery in the Light of Political Science," in E. N. Elliott, ed., *Cotton Is King, and Pro-Slavery Arguments* (Augusta, Ga., 1860), 634–746.

<sup>30</sup> Marwitz, "Kritik des Steinschen Testaments," in Friedrich Meusel, ed., *Friedrich August Ludwig von der Marwitz: Ein märkischer Edelmann im Zeitalter der Befreiungskriege*, 2, pt. 1 (Berlin, 1913): 240, 242.





*Figure 1: James Henry Hammond, Southern planter, in the uniform of the commander in chief of the South Carolina Militia, a rank he held as governor of South Carolina. Photograph from a copy of a painting by Gerald Foster, ca. 1842, reproduced courtesy of South Caroliniana Library.*

of the Prussian working classes, Dohna-Kotzenau responded to traditionalists who extolled the pre-1807 master-serf relationship:

It is ironic that this relationship has been labeled patriarchal. Its participants were not united into a family by indissoluble bonds; the dependents were chained indissolubly only to the authority of the *Rittergut*, whose owner could sell the estate to a new lord without hindrance and, with it, the dependents. The children awaken one morning and discover that their father has sold them, that they have a new father. Is this the subject for a family portrait? . . . Nor have I spoken here of a lord who was tyrannical toward his dependents but of a kind, considerate lord who lightened the labor obligations of his subordinates when possible and who maintained them in misfortune; but I believe I have shown that he could only be a kind lord and that he stood in too high and isolated a position to be called the father of a family.<sup>31</sup>

Applying the logic of Dohna-Kotzenau's argument to the master-slave relationship in the Old South raises noteworthy semantic objections to any scholar's use of the terms "patriarchal" or "paternalistic" to characterize the institution of slavery. In the first place, their etymologies have imbued these adjectives with strong connotations of familial attachment and fatherly concern, which seem to

<sup>31</sup> Hermann Graf zu Dohna-Kotzenau, *Die freien Arbeiter im preussischen Staate* (Leipzig, 1847), 10–11.





Figure 2: Friedrich August Ludwig von der Marwitz, Prussian Junker, in the uniform of a Prussian cavalry general (ret.), and marshal of the Brandenburg Provincial Landtag. Photograph from a painting by Franz Krüger, ca. 1827, taken from Walter Görlitz, *Die Junker* (1964), and reproduced courtesy of C. A. Starke Verlag.

survive all social scientific efforts to provide them with ethically neutral definitions.<sup>32</sup> More significant, to evaluate slavery as an institution with language that recalls the planters' own flattering appraisal of "domestic servitude" is to compound the difficulties inherent in the historian's obligation to distinguish clearly between the images historical actors had of themselves and the reality of their historical situation.<sup>33</sup> Although many a planter, like many a Junker, viewed his

<sup>32</sup> Anthropologist John W. Bennett has explained that "the derivation of the term 'paternalism' from a Latin-English kinship term suggests its root meaning: a type of behavior by a superior toward an inferior resembling that of a male parent to his child—in most cases, a son." Hence Bennett has found it necessary to distinguish clearly between "benevolent" and "exploitative" modes of paternalistic action. Bennett, "Paternalism," in *International Encyclopedia of the Social Sciences*, 2 (2d ed., New York, 1968): 472, 472–77.

<sup>33</sup> In *Roll, Jordan, Roll* Eugene Genovese made a concerted but tortuous effort to identify "paternalism" as the definitive characteristic of Southern slavery, while distinguishing between "paternalism" as the planters understood it and "paternalism" as the slaves themselves allegedly understood it. Nonetheless, the term "paternalism" still seems incongruous with a system of labor relations, the logic of which, Genovese claimed, "pushed the masters to break their slaves' spirit and to reconstruct it as an unthinking and unfeeling extension of their own will." Only "the slaves own resistance to dehumanization compelled the masters to compromise in order to get an adequate level of work out of them." *Roll, Jordan, Roll*, 317. My argument on this issue also puts me somewhat at odds with another formidable scholar—Max Weber, whose research on nineteenth-century Junkers and their labor force has been so valuable to German historians. In 1906 Weber observed, "In the first half of the last century the Junker was a rural patriarch"; Weber, "Capitalism and Rural Society in Germany," in Weber, *From Max Weber: Essays in Sociology*, ed. H. H. Gerth and C. Wright Mills (New York, 1946), 382.

position as that of a kindly paterfamilias tending to the welfare of his faithful “people,”<sup>34</sup> the actual interaction between a planter and his laborers seldom approximated this patriarchal ideal—especially when the “people” were field hands rather than house servants or skilled artisans. The Mississippi planter Cornelius Vanhauten composed a will in 1850 that indicates how the financial constraints of chattel slavery could inhibit the paternalistic intentions of even an unusually benevolent slaveholder: “4th. I direct and empower my Executors to sell all my Negro Slaves with the exception of Joe hereafter to be disposed of at private sale to humane masters provided it can be done to advantage near the valuation fixed by my appraisers but in no event to separate families.”<sup>35</sup>

Although it seems inappropriate to characterize the relations between either planters and their slaves or Junkers and their cottagers as “patriarchal” or “paternalistic,” the need of both landed elites to obtain productive labor from sentient human beings, combined with the ethical dictates of Christianity, induced both planters and Junkers alike to seek the cooperation of their workers through more positive incentives than fear and compulsion. In fact, landowners distinguished more by self-interested practicality than by altruistic benevolence often stressed that considerate treatment of slaves or estate laborers could yield substantial dividends in terms of worker efficiency and loyalty. In 1842, Judge John Belton O’Neill of South Carolina claimed to members of his state’s agricultural society, “You cannot succeed with negroes, as operatives, as you desire to do, *unless you feed and clothe [them] well*. —Make them contented, and then ‘Massa’ will be, as he ought to be, the whole world to them.”<sup>36</sup> And, in 1849, an East Prussian estate owner noted in the Berlin agricultural journal, *Annalen der Landwirtschaft*, “In general, the cottagers, if they are well treated and also supervised, are capable and not incompetent workers who perform more work on their poor diet than one ought to expect; one must seek only to maintain their good will and fresh spirit. Without supervision and with bad treatment they accomplish little and do not take adequate care of property.”<sup>37</sup>

Southern planters, of course, had yet another and often more compelling incentive to treat their slaves with at least a modicum of benevolence—the huge capital investment that plantation labor, unlike *Rittergut* labor, constituted. A slaveholder could expect to reap substantial capital gains from the increase in both the market value and the number of his slaves. Andrew Flynn of Mississippi went so far as to advise his overseer that “the children must be very partic-

<sup>34</sup> Franz Rehbein (1867–1909) worked on a Pomeranian *Rittergut* as a boy, and his autobiography, originally published in 1911, provides vivid testimony about the self-consciously patriarchal bearing of the estate owner toward his “people” (*Leute*) as late as the 1880s; see Rehbein, *Das Leben eines Landarbeiters*, ed. Karl Winfried Schafhausen (Darmstadt, 1973), 52. In regard to planters, Genovese has stated, “The slaveholders’ vision of themselves as authoritarian fathers who presided over an extended and subservient family, white and black, grew up naturally in the process of founding plantations”; *Roll, Jordan, Roll*, 74. Much the same argument could be made for the Junkers.

<sup>35</sup> Vanhauten, Manuscript Will (Draft), October 9, 1850, in the Richard Abbey Papers, Mississippi State Department of Archives and History, Jackson, Miss.

<sup>36</sup> O’Neill, “Judge O’Neill’s Address Delivered before the South Carolina Agricultural Society, December 29, 1842,” *Southern Cultivator*, 1 (1843): 109.

<sup>37</sup> Gutsbesitzer Lüdersdorf auf Christoplack, “Die landwirtschaftlichen Verhältnisse des Labiauer Kreises, in Reg.-Bezirk Königsberg,” *Annalen der Landwirtschaft*, 13 (1849): 129–30.

ularly attended to, for rearing them is not only a duty, but also the most profitable part of plantation business." Hence, there was some truth in the contention of Dr. William C. Daniell of Georgia that planters were "the only capitalists whose system embraces humanity to the laborer consistently with economy of investment."<sup>38</sup>

PLANTERS AND JUNKERS ARE COMPARABLE primarily because of certain similarities between the antebellum plantation and *Vormärz Rittergut* as commercial enterprises and political communities. There were, however, vast dissimilarities between their respective historical environments. Though the plantation and the Junker estate were both hierarchical and authoritarian institutions, the larger society within which the planters defended slavery was less hierarchical and more fluid, less authoritarian and more democratic, than the larger society within which the Junkers defended the privileges of *Rittergut* ownership. Successful, constructive use of the comparative method certainly does not require proving that analogous phenomena were of greater historical significance than the differences between their environments. As Marc Bloch stated, the comparative method may contribute primarily to the isolation and analysis of "the 'originality' of different societies."<sup>39</sup>

The Prussian state during *Vormärz* was a hereditary monarchy whose power rested primarily upon a strong standing army and an extensive civil bureaucracy. Since the mid-seventeenth century the maintenance and expansion of the army—an absolute necessity for the monarchy in view of its precarious position on the plains of north-central Europe—had encouraged the growth of a professional state bureaucracy, which in time became an independent power in its own right and, under the Civil Code (*Allgemeines Landrecht*) promulgated in 1794, even obtained its own legally privileged status in Prussian society. Eighteenth-century Junkers generally found service as army officers more congenial than employment as bureaucrats, in part because of their rustic life-style and poor education and in part because of the ease of stepping from exercising patrimonial authority over peasants on the *Rittergut* to wielding military command over peasants on the battlefield. Frederick William I and Frederick the Great made the officers' corps a virtual Junker preserve, thereby serving the interests both of less affluent Junker families and of Prussian monarchs anxious to secure the loyalty of their parochial landed aristocracy. Within the civil service, however, the regular recruitment of bureaucrats from outside Junker ranks fostered the development of socioeconomic and political values different from and sometimes even antagonistic to those of landed Junkerdom. Although Prussia's military humiliation at the hands of Napoleonic France in 1806 provided the immediate impetus for the Stein-Hardenberg reforms, the reform statutes were

<sup>38</sup> Flynn, 1840 Plantation Rules, Andrew Flynn Plantation Book (microfilm), in the Southern Historical Collection, University of North Carolina, Chapel Hill, N.C.; and Daniell, "An Address" [to a convention called to organize an Agricultural Association of the Slaveholding States, Montgomery, Ala., May 2, 1853], *American Cotton Planter*, 2 (1854): 66.

<sup>39</sup> Bloch, "Toward a Comparative History," trans. Riemersma, 507.

conceived and implemented by a bureaucracy that contained many educated "commoners" and non-Junker noblemen (such as ministers Stein and Prince von Hardenberg, for example, who were originally from Nassau and Hannover). These reformers tended to look upon the "boundness" of the peasantry and the Junkers' traditional corporate privileges as antiquated obstacles to the growth of Prussian state power, the increased vitality of Prussia's economy, and the evolution of a social order founded on the classical liberal principle of "careers open to talent."

Nonetheless, the reformers' respect for the sanctity of private property and their distrust of political democracy prevented them from attempting any wholesale reconstruction of Prussian society, much as the victorious North's regard for property rights and distrust of the Negro freedmen prevented the federal government from attempting a wholesale reconstruction of Southern society after the Civil War. Indeed, as has been suggested above, the Stein-Hardenberg reforms had the effect of strengthening the economic vitality of *Rittergut* agriculture prior to 1848 and thereby "made it possible for the aristocracy, as a class, to maintain and strengthen itself."<sup>40</sup> This economic rejuvenation was, moreover, accompanied by a partial social and political reconciliation between landed Junkerdom and the upper ranks of the bureaucracy, as increasing numbers of Junker sons attended universities and entered the civil service and as senior officials realized that the smoldering movement for civil equality and constitutional government posed a threat to their own privileged status under the Civil Code as well as that of the Junkers.<sup>41</sup> The extent of this Junker-bureaucrat rapprochement became apparent during the revolutions of 1848–49, when the higher echelons of the civil service rejected democracy and radicalism with as much fervor as did most Junkers, while the army and its aristocratic officers' corps constituted the rock of monarchical loyalty on which the wave of revolution eventually broke. Thus, the Junkers' "privileged position next to the bureaucracy and the military in the Prussian power syndicate,"<sup>42</sup> in conjunction with their shrewd adaptability as agrarian entrepreneurs, enabled them to be a tenacious and formidable landed elite throughout the nineteenth and early twentieth centuries.

The Junkers' involvement with military and bureaucratic absolutism and their skill as agrarian capitalists did not preclude their holding fast to the traditional corporatist view of society as a hierarchical and "organic" structure of distinct social groupings—the *Stände* ("estates")—topped, of course, by the large landowners.<sup>43</sup> This conception of Prussian society was a legacy from the pre-ab-

<sup>40</sup> Koselleck, *Preussen zwischen Reform und Revolution*, 85.

<sup>41</sup> "The influx of the impoverished lesser nobility into middle and upper bureaucratic positions also strengthened the bureaucracy's feudal perceptions"; Walter Görlitz, *Die Junker: Adel und Bauer im deutschen Osten* (3d ed., Limburg a/L., 1964), 229. Also see, for a clear and concise analysis, John R. Gillis, "Aristocracy and Bureaucracy in Nineteenth-Century Prussia," *Past & Present*, no. 41 (1968): 107–19.

<sup>42</sup> Hans-Jürgen Puhle, "Aspekte der Agrarpolitik im 'Organisierten Kapitalismus,'" in Hans-Ulrich Wehler, ed., *Sozialgeschichte Heute: Festschrift für Hans Rosenberg zum 70. Geburtstag* (Göttingen, 1974), 547.

<sup>43</sup> For a concise definition of *Stand* from a traditional perspective, see "Stand" in *Dr. Johann Georg Krünitz's ökonomisch-technologische Encyclopädie*, cont. Johann Wilhelm David Korth, 169 (Berlin, 1838): 604–05. Also see Robert M. Berdahl, "The *Stände* and the Origins of Conservatism in Prussia," *Eighteenth-Century Studies*, 6 (1973): 298–321.

solutist conditions of the fifteenth to seventeenth centuries, when the members of the landed aristocracy (*Ritterstand*) tended to look upon their Hohenzollern prince as *primus inter pares*. During *Vormärz* the corporatist view of society served to justify the Junkers' privileged representation in and political dominance over both the county and provincial assemblies (*Kreistage* and *Landtage*), which were important to *Rittergut* owners primarily as institutional counterweights to the "mechanistic" power of the bureaucracy.<sup>44</sup> Even after the revolutions of 1848–49 introduced quasi-parliamentary constitutionalism to Prussia, Junker spokesmen like Friedrich Julius Stahl argued that the landed interest was entitled to privileged electoral representation in the national legislature as the country's most important occupational estate (*Berufstand*).

In the American South slavery and the plantation regime did instill some military values in planter society, and many planter sons pursued careers as officers in the U.S. Army. But the geographical situation of the American colonies and states, like that of England, obviated the need for a strong standing army; and the absence of such an army precluded the pre-industrial development of a centralized bureaucracy such as military absolutism fostered in Prussia. Antebellum planters actively participated and firmly believed in republican and parliamentary rather than monarchical and authoritarian government. Most planters—even those in older, more "aristocratic" areas like the Chesapeake Tidewater and the lower Mississippi Valley—adapted successfully, if unwillingly, to the democratization of political style and structure that occurred during the era of Jackson and the second (or Jacksonian) party system. From the mid-1830s to the mid-1850s the national two-party system flourished in all of the Southern states except Calhoun's South Carolina, although both Whig and Democratic politicians in the South were anxious to present their respective parties as "sound" on slavery.<sup>45</sup>

But the antebellum South increasingly became a minority section within the federal Union, while pre-1848 East Elbia remained the preponderant heartland of the Prussian monarchy. The 1850s witnessed an accelerating decline of the South's electoral strength within the federal government, as the free states rapidly outstripped the slave states in both number and population. During the mid-1850s the sharpening sectional conflict over slavery's westward expansion into the territories and the rise of an antislavery party in the North served to destroy the Jacksonian party system; the Democratic Party seemed to be the South's only hope of protecting slavery within the Union. Even the "Democracy" was torn apart during the spring and summer of 1860 by fierce quarreling between the party's Northern and Southern wings over the status of slavery in the territories. Until Lincoln's election in November, however, most planters abjured secession and believed that loyalty to the Constitution—properly inter-

<sup>44</sup> On the importance to Old Prussian conservatism of a corporate organization of society in general and of the *Kreistage* and *Landtage* in particular, see Carl Wilhelm von Lancizolle, *Ueber Königtum und Landstände in Preussen* (Berlin, 1846), esp. 242–50, 461–70. On the county and provincial assemblies during *Vormärz*, see Hefner, *Die deutsche Selbstverwaltung im 19. Jahrhundert*, 129–32.

<sup>45</sup> See, in particular, William J. Cooper, Jr., *The South and the Politics of Slavery, 1828–1856* (Baton Rouge, 1978), esp. 50–74.



puted—was the surest defense of both their property rights (ensured by the Fifth Amendment) and their political power (dependent on the three-fifths rule and Southern representation in the Senate). The election of a Republican president, on a platform that most Southerners (and certainly most planters) saw as threatening the ultimate abolition of slavery, drove the seven states of the Lower South from the Union; and Lincoln's "coercive" call in April 1861 for seventy-five thousand troops to subdue the "rebellion" provoked the secession of four more states in the Upper South.

It is important to emphasize that the splits in the Southern electorate *before* the November presidential election—among supporters of Breckinridge, Bell, and Douglas—and the splits *after* the election returns—among separate-state secessionists and the several varieties of "cooperationists"—reflected disagreement over what political strategy would serve best to protect slavery, not disagreement over the fundamental necessity to preserve the institution. As Dwight Lowell Dumond demonstrated decades ago, "At the South, the members of all political parties were dedicated to the preservation of the institution of slavery."<sup>46</sup> In order to understand how planters and plantation slavery were so compatible with political democracy in the Old South, it is valuable to consider some geographic, demographic, and racial differences between the South and East Elbia.

Geographic and demographic conditions in the antebellum South provided many more opportunities for upward social mobility than could exist in *Vormärz* eastern Prussia. East Elbia contained much less uncultivated land than did the Southern states, and it did not have an expanding frontier. With a total land area not as large as that of Louisiana and Mississippi combined, East Elbia in 1849 supported a population nearly as large as that of all fifteen slave states in 1860.<sup>47</sup> The continuing availability of unsettled land in the Old South, in conjunction with the profitability of cotton-growing, gave whites opportunities for social as well as geographical mobility that were impossible in pre-1848 Prussia. Just as ambitious yeomen farmers could realistically hope to become small slaveholders, ambitious lesser slaveholders could strive to become gentlemen planters.<sup>48</sup> The antebellum planter class expanded both numerically and geo-

<sup>46</sup> Dwight Lowell Dumond, *The Secession Movement, 1860–1861* (1931; reprint ed., New York, 1973), 98. With respect to Georgia, Michael P. Johnson has mistakenly assumed that, in the election of delegates to the state secession convention on January 2, 1861, votes in favor of delegates committed to cooperative action among the slave states vis-à-vis the Republican North, as opposed to votes for delegates committed to separate-state secession, reflected in many instances a lack of commitment to the preservation of slavery; Johnson, *Toward a Patriarchal Republic: The Secession of Georgia* (Baton Rouge, 1977), 65–93. Cooperationists throughout the South included in their number wealthy slaveholders as well as nonslaveholders.

<sup>47</sup> The total land area of East Elbia was 90,283 square miles in the 1850s; that of the state of Mississippi, 47,240, and that of Louisiana, 45,177 (for a total of 92,417 square miles). East Elbia's population in 1849 was just over twelve million against twelve million three hundred thousand (white and black) for all fifteen slave states in 1860.

<sup>48</sup> Even though the ratio of slaveholders to the South's total free population declined slightly between 1790 and 1850 and then dropped substantially during the 1850s, the absolute number of slaveholders continued to climb until the Civil War—from 347,525 in 1850 to 384,884 in 1860, an increase of more than 10 percent. Moreover, the number of "planters" owning twenty to fifty slaves increased from 29,733 in 1850 to 35,623 in 1860, an increase of almost 20 percent. In 1860, 31 percent of white families in the eleven states that formed the Confederacy owned slaves. Otto Olson has explained that, "while 31 per cent may not appear large as a voting or even [an] isolated ownership statistic, it is enormous if . . . slavery is viewed as the economic founda-



graphically, while in *Vormärz* East Elbia the fixed number of increasingly expensive *Rittergüter* could only change hands among the wealthy upper crust.<sup>49</sup> Consequently, the number of Prussians who could hope to become part of landed Junkerdom was proportionately far smaller than the number of Southerners who could aspire to the status of gentlemen planters. If only for this reason, the planters enjoyed a much higher degree of popular legitimacy than did the Junkers.<sup>50</sup> Accordingly, the planters of older, more settled states had at least a threefold interest in securing new territory for the expansion of the plantation system: (1) to enlarge the South's representation in Congress by creating new slave states; (2) to guarantee a steady market for their surplus slaves, which would maintain and even increase the market value of the slaves they retained and, by exporting excess Negroes, reduce the possibility of a successful slave rebellion; and (3) to assure the continued availability of cheap, fresh land for generations of aspiring planters, whose aspirations were instrumental in legitimizing plantation society.<sup>51</sup>

Another crucial difference between the historical environments of the two landed elites—and perhaps the most important—was racial. The South was a racially bifurcated society in which the enslavement of Negroes and anti-Negro prejudice had fed on each other since the seventeenth century. But the Junkers never enjoyed the advantage of a labor force isolated by race from the population at large; they therefore had much more to fear than did the planters from an indigenous popular movement for democratic reforms. In the aftermath of 1848–49, Count Friedrich zu Dohna-Lauck of East Prussia wrote of “the lack of political judgment and the gullibility of the lower classes” and of the consequent “necessity for the complete exclusion of the working class and those not economically self-sustaining from any participation in the election of representatives of the country.” Dohna-Lauck saw the plutocratic three-class suffrage system de-

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tion of an entire social system and the distribution of slaves is compared to analogous factors in a free society.” Olson pointed out that in 1949 only 2 percent of American families owned stock worth \$5,000 or more, an investment comparable to the ownership of one slave in 1860. Olson, “Historians and the Extent of Slave Ownership in the Southern United States,” *Civil War History*, 18 (1972): 111–12.

<sup>49</sup> Editor Carl Sprengel reported that in 1840 *Rittergüter* in eastern Pomerania, which had sold in the late 1820s for 50,000 *Taler*, were selling for 100,000 to 150,000 *Taler*; Sprengel, “Landwirtschaftlicher Bericht aus Hinterpommern. Anfang April 1840,” *Allgemeine Landwirtschaftliche Monatschrift*, 1 (1840): 144. Estate owner Lüdersdorf auf Christoplack reported that an estate in a neighboring East Prussian county (*Kreis*) had sold in 1834 for 11,000 *Taler*, in 1841 for 24,000 *Taler*, and in 1849 for 44,000 *Taler*; “Die landwirtschaftlichen Verhältnisse des Labiau Kreises,” 116. In the mid-1850s, one Prussian *Taler* was the equivalent of approximately \$0.70 in U.S. currency; I have computed this equivalence from figures in Georg Friedrich Kolb, *Handbuch der vergleichenden Statistik der Völkerzustands- und Staatenkunde* (Zurich, 1857), 127, 314.

<sup>50</sup> Alexis de Tocqueville made an incisive observation about the English aristocracy, which is also applicable to the planter elite. The English aristocracy, he wrote in 1833, was “not in the least founded on birth, but on the wealth that everyone can acquire”; and, “as everyone had the hope of being among the privileged, the privileges made the aristocracy not more hated, but more valued”; Tocqueville, “Journey to England,” in Tocqueville, *Journeys to England and Ireland*, ed. J. P. Mayer (New Haven, 1958), 59. In the words of Robert S. Starobin, “Between poor whites and slaveless yeomen on the one hand and planters on the other there was often a degree of hostility, but class conflict seldom became serious so long as economic opportunities for ambitious whites seemed open”; Starobin, *Industrial Slavery in the Old South* (New York, 1970), 6.

<sup>51</sup> On the interests of Southern yeomen farmers and small slaveholders in securing new land for the expansion of slavery, see Gavin Wright, *The Political Economy of the Cotton South: Households, Markets, and Wealth in the Nineteenth Century* (New York, 1978), 33; and William R. Brock, *Parties and Political Conscience: American Dilemmas, 1840–1850* (Millwood, N.Y., 1979), 236–37, 274.

creed by the Manteuffel ministry in May 1849 as only the first step in the development of a corporate scheme of representation under which large landowners would enjoy their traditional pre-eminence.<sup>52</sup> In the South black slavery practically eliminated for the planters the troublesome problem of a free agricultural working class whose poverty and exclusion from political participation inspired social and political agitation. Charleston's *Southern Quarterly Review* explained in 1853 that "the mass of mankind constitute the labouring class, and are, perforce, required to live by the sweat of the brow." Given this condition, the South was fortunate to have a working class composed of those "who are not pampered with false notions of their own claims to liberty and unnatural elevation."<sup>53</sup>

Planters could preach and even practice political democracy among whites because their labor force was excluded by race from the political process and because nonslaveholding whites could and did share in the planters' racial contempt for the deprived and depraved Negro.<sup>54</sup> While the Junkers believed that a corporate structuring of political life was necessary to preserve their status as a landed elite, the planters could participate and even predominate in what has been called a *Herrenvolk* democracy—that is, a democracy for the master race.<sup>55</sup> Thus, Alabama Governor and Black Belt planter John A. Winston could declare in 1855, during his second inaugural address, that "the existence of a race among us—inferior by nature to ourselves, in a state of servitude—necessarily adds to the tone of manliness and character of the superior race."<sup>56</sup>

IN CONSIDERING THE COMPARABILITY OF PLANTERS AND JUNKERS as "conservative" landed elites, some clarification of the historical concept represented by the slippery words "conservative" and "conservatism" is in order. Like all historical concepts, this one should be understood as an analytical rather than a descriptive tool, an ideal-typical construct that can help make sense of complex histori-

<sup>52</sup> Dohna-Lauck, *Ueber die nothwendige Abänderung der beiden Wahlgesetze für die erste und die zweite Kammer: Eine Petition an die hohen Kammern* [July 30, 1849] (Königsberg, 1849), 5–6, 15, 18–33.

<sup>53</sup> [James Chestnut, Jr.] "The Destinies of the South," *Southern Quarterly Review*, 23 (1853): 196–97. (William Gilmore Simms was editor of the *SQR* from 1849 to 1854.) Several years later this same periodical published a review of George Fitzhugh's *Sociology for the South* (1854) and therein expressed the feelings of the vast majority of proslavery conservatives by refusing "to go so far as Mr. Fitzhugh and maintain that nothing but slavery can be right"; George Frederick Holmes, "Slavery and Freedom," *Southern Quarterly Review*, 29 (1856): 71. Holmes was a migrant academic, who gave a similar response to Fitzhugh, also emphasizing that Southern slavery was founded on the racial inferiority of blacks rather than on the merits of slavery *per se*, in another periodical; see Holmes, "Failure of Free Societies," *Southern Literary Messenger*, 21 (1855): 132. According to Eugene Genovese, however, "the notion that slavery was a proper system for all labor, not merely for black labor, . . . grew steadily as part of the growing self-awareness of the planter class," and Fitzhugh provided "a more rigorous and mature presentation of a line of thought which had been gaining steadily for years"; *The World the Slaveholders Made*, 130.

<sup>54</sup> See the suggestive observations in Edmund S. Morgan, *American Slavery, American Freedom: The Ordeal of Colonial Virginia* (New York, 1975), 380–86.

<sup>55</sup> Pierre L. van den Berghe, *Race and Racism: A Comparative Perspective* (2d ed., New York, 1978), 18, 29; George M. Fredrickson, *The Black Image in the White Mind: The Debate on Afro-American Character and Destiny, 1817–1914* (New York, 1971), 61–68; and Kenneth P. Vickery, "'Herrenvolk' Democracy and Egalitarianism in South Africa and the U.S. South," *Comparative Studies in Society and History*, 16 (1974): 309–28.

<sup>56</sup> Winston's address was published in the *Richmond Whig*, January 4, 1856, with an editorial note commending the governor's "just views" to the paper's readers.

cal developments. As such, it transcends any specific historical context, but the use of such a construct carries the presumption that every manifestation of conservatism is the product of a particular historical milieu.<sup>57</sup> Its transcendence is based on the premise that being “conservative” entails defending the status quo before a “progressive” attack demanding change. Occasionally, individuals may exhibit a conservative disposition to distrust and may resist change for reasons whose explanation belongs more to the realm of personal psychology than to the sphere of historical analysis. But a historically significant manifestation of conservatism—a conservative movement—emerges only when members of a substantial social group or community are confronted by a serious “progressive” challenge to the values and institutions they have previously taken for granted. “Reactionary” ideas and sentiments can often serve conservative purposes, in that the reactionary’s romantic glorification of a bygone era can have the effect of discrediting progressive change in the present.

An elitist variety of conservatism emerges when a highly influential and prestigious minority group faces a potent reformist threat to those established, “traditional” institutions and values that are essential to its favored position in society. An elitist conservative movement, like the progressive challenge that provokes it, is both a political and an intellectual phenomenon and gives rise to complex theoretical justifications of the status quo—that is, a multifaceted conservative ideology. Conservative ideologues, of course, constitute only a small proportion of the elite’s members; indeed, some of these ideologues are well-educated “outsiders” who fulfill an intricate blend of social, material, and intellectual ambitions by identifying their lives and ideas with the elite’s influence and prestige.

Proslavery conservatism in the South and Old Prussian conservatism in East Elbia developed in response to substantive reformist attacks on both the fundamental interests and the self-esteem of the planters and Junkers. In each case the progressive ideology was a mixture of democratic, free-labor, and humanitarian impulses, which shared an intolerance of planter “aristocrats” and Negro servitude in the South or of “feudal” Junkers and oppressed *Rittergut* labor in East Elbia. Although the conservative ideas advanced by proslavery and Old Prussian ideologues did serve as occasional weapons in political struggles to protect the concrete interests of the two landed elites, these ideas also helped satisfy the personal desires of many planters and Junkers for public vindication before the frequent invectives of progressive critics. A widespread proslavery movement in Southern thought and politics developed between the Missouri crisis of 1819–21 and the controversies over abolitionist mailings and petitions of 1835–37. During these years the planters’ apprehensions that Congress’s Northern majority would interfere with the expansion of slavery were augmented by their more in-

<sup>57</sup> My approach to the concept of conservatism has been influenced by Karl Mannheim, “Conservative Thought” (1927), trans. Paul Kecskemeti and Kurt Wolff in Mannheim, *From Karl Mannheim*, ed. Kurt Wolff (New York, 1971), 132–222; Sigmund Neumann, *Die Stufen des preussischen Konservatismus: Ein Beitrag zum Staats- und Gesellschaftsbild Deutschlands im 19. Jahrhundert*, Historische Studien, no. 190 (Berlin, 1930), esp. 7–12; Samuel P. Huntington, “Conservatism as an Ideology,” *American Political Science Review*, 51 (1957): 454–73; and Hans-Gerd Schumann, “The Problem of Conservatism: Some Notes on Methodology,” *Journal of Contemporary History*, 13 (1978): 803–17.

tense fears that abolitionist agitation would set free the demon of slave rebellion.<sup>58</sup> Old Prussian conservatism emerged in response to the “revolution from above” that the Junkers confronted during the Stein-Hardenberg Reform Era of 1807–22.<sup>59</sup> At this time the bureaucracy not only ended serfdom and the nobility’s de jure monopoly over the ownership of *Rittergüter* but even threatened, albeit unsuccessfully, to undermine the Junkers’ *Rittergut*-based combination of large estates with autocratic rights of local government. Since *Vormärz* Junkers reacted to the actual abolition of serfdom, while antebellum planters confronted only a threat to slavery, Old Prussian ideology included a pronounced reactionary strain that was absent from proslavery thought. Proslavery ideology, moreover, was much less overtly elitist, and much more regional, than was its Old Prussian counterpart, since planters and slavery enjoyed a higher level of popular legitimacy than did Junkers and privileged *Rittergüter*. It was no accident, however, that both the Southern proslavery and the Old Prussian ideologies first unfolded in older states or provinces with relatively entrenched planter or Junker elites—South Carolina and Virginia, Brandenburg and Pomerania.

The comparability of the conservatism of antebellum planters and *Vormärz* Junkers does not rest solely on the progressive challenge to the status quo that each landed elite perceived. It also rests on the analogous economic, social, and political institutions—the “traditional” institutions—that, in the final analysis, the two landed elites defended: the plantation with its enslaved work force and the *Rittergut* with its dependent laborers. The continuing economic vitality of these two institutions and their owners served to prolong the exclusion of large segments of the American and Prussian populations from civil and political equality; and the proprietor of a plantation or *Rittergut* was entitled to wield a species of private authority that is incompatible with the democratic nation-state.<sup>60</sup> If the United States and the Kingdom of Prussia during the first half of the nineteenth century were public law states, then the plantation and *Rittergut* were miniature private law states within which the planter and Junker exercised

<sup>58</sup> By the mid-1830s some Southerners—South Carolinians in particular—had developed the argument that slavery was a “positive good” rather than a “necessary evil”; William Sumner Jenkins, *Pro-Slavery Thought in the Old South* (1935; reprint ed., Gloucester, Mass., 1960), 65–81; and William W. Freehling, *Prelude to Civil War: The Nullification Controversy in South Carolina, 1816–1836* (New York, 1965), 76–82, 327–33. Both arguments, “positive good” and “necessary evil,” had their adherents among Southern planters during the three decades before the Civil War, although the former certainly dominated public discussion of the slavery issue below the Mason-Dixon Line during the 1850s. As William Cooper, Jr., has observed, it is impossible to say how many Southerners “accepted or believed” the argument that slavery was a “positive good” instead of the argument from necessity. “In practical terms,” however, “it mattered little whether or not a southerner accepted or rejected the argument that slavery was a positive good,” since those who favored the argument from necessity also insisted that the South must deal with its peculiar institution free from outside interference; *The South and the Politics of Slavery*, 60–63.

<sup>59</sup> The term “Old Prussian,” a translation of the adjective *altpreussisch*, is used in Rosenberg, *Bureaucracy, Aristocracy, and Autocracy*, 221–28. During and after the period of the Stein-Hardenberg reforms, *altpreussisch* became a historical synonym for the adjectives *hochkonservativ* (“high conservative”) and *ständisch-konservativ* (“corporate-conservative”). Ernst Huber has used these two terms exclusively in discussing conservatives in the 1840s, those whom I call Old Prussian; Huber, *Deutsche Verfassungsgeschichte seit 1789*, vol. 2: *Der Kampf um Einheit und Freiheit, 1830 bis 1850* (Stuttgart, 1960), 331–39.

<sup>60</sup> On the incompatibility of the planter’s or Junker’s private autocratic authority with the democratic nation-state, see William H. McNeill, *The Rise of the West* (New York, 1965), 815–16; and Reinhard Bendix, *Nation-Building and Citizenship: Studies in Our Changing Social Order* (rev. ed., Berkeley and Los Angeles, 1977), 122, 128.

almost sovereign authority. From this perspective, planter apologists defending slavery were just as conservative, in a qualitative sense, as were Junker apologists defending the authority of the *Rittergut* owner over his menials. Plantations, planters, and proslavery conservatism in the South and *Rittergüter*, Junkers, and Old Prussian conservatism in East Elbia constituted the major breakwaters in the United States and Prussia during the nineteenth century against what can be called the liberal main currents of Western thought and politics since the American and French Revolutions—that is, against the increasingly popular arguments for and the gradual and halting movement toward civil and political equality, which in the twentieth century has become a campaign for social and economic equality as well.<sup>61</sup>

Proslavery and Old Prussian ideologues made extensive use of conservative principles associated with Edmund Burke and his *Reflections on the Revolution in France* (1790). “Burkean principles” were exceedingly well suited to an ideological defense of both the plantation with its slaves and the Junker estate with its dependent cottagers and servants. Although political scientist Louis Hartz has argued that the Old South’s invocation of Burke was fraudulent, because antebellum conservatives defended not feudalism but slavery,<sup>62</sup> the British society that Edmund Burke defended was far from feudal.<sup>63</sup> On the contrary, with the possible exception of the Netherlands, England enjoyed the most “liberal” and “capitalistic” civilization in eighteenth-century Europe.

Burkean conservatism can be reduced to two fundamental principles. The first is the necessity for historical continuity, which entails respect for established

<sup>61</sup> My use of the terms civil, political, and social equality follows that of T. H. Marshall, “Citizenship and Social Class” (1949), in Marshall, *Class, Citizenship, and Social Development: Essays by T. H. Marshall* (Garden City, N.Y., 1965), 78. I suppose an ideal-typical nineteenth-century conservative in the West would have reacted against the manifold effects of what Eric Hobsbawm has identified as the “dual revolution” of the late eighteenth century—that is, “the French Revolution of 1789 and the contemporaneous (British) Industrial Revolution”; Hobsbawm, *The Age of Revolution: Europe, 1789–1848* (London, 1962), xv. In their historical interaction, the two facets of this dual revolution have been the primary agents of what has been called the process of modernization; see Reinhard Bendix, “Tradition and Modernity Reconsidered,” in his *Embattled Reason: Essays on Social Knowledge* (New York, 1970), 294–96. Bendix has written more recently that the “term modernization is applied best where nonindustrial ways of life and hierarchical social orders are threatened by industrial ways and egalitarian social norms”; Bendix, *Kings or People: Power and the Mandate to Rule* (Berkeley and Los Angeles, 1978), 12. It can be argued that antebellum planters and Vormärz Junkers were “antimodern” according to Bendix’s ideal-typical formulation; but these two landed elites felt themselves to be—and in fact were—far less threatened by industrialization than by the democratic and egalitarian impulses that were a legacy of the American and French Revolutions. The cooperation between Junkers and industrialists that evolved in Prussia after 1848 shows that conflict between agrarian and industrial elites in the nineteenth century was neither intrinsic nor irrepressible. The major obstacle to the development of such peaceful cooperation between antebellum Southern planters and Northern industrial interests was the accelerating controversy over slavery’s spread into the western territories. As Barrington Moore has pointed out, plantation slavery was not “an economic fetter upon industrial capitalism,” but rather “an obstacle to a political and social democracy”; *Social Origins of Dictatorship and Democracy*, 112. Unfortunately, however, Moore has ignored the important racist components of the North’s opposition to the expansion of slavery.

<sup>62</sup> Hartz, *The Liberal Tradition in America*, 146–47.

<sup>63</sup> According to Harold Perkin, “Feudalism as a social system in England disappeared with the fourteenth- and fifteenth-century commutation of labor services”; Perkin, *The Origins of Modern English Society, 1780–1880* (Toronto, 1969), 25. R. S. Neale has lent support to Perkin’s statement in observing that by the early sixteenth century English agriculture “was largely a specialised and market-oriented agriculture, literally dominated in parts of the west country by large-scale capitalist farming.” The English landowner, Neale has suggested, “experienced a substantial dose of embourgeoisement well before the bourgeoisie existed.” Neale, “The Bourgeoisie, Historically, Has Played a Most Revolutionary Part,” in Eugene Kamenka and R. S. Neale, eds., *Feudalism, Capitalism, and Beyond* (London, 1975), 92, 94.



property rights and regard for tradition and experience over abstract reason and theory as guides to social and political policy; this regard for tradition and experience is imperative because human nature is too sinful and human reason too flawed to be relied upon as the fonts of wisdom. The second principle is the belief that the hierarchical structure and inegalitarian values of established society are in accord with divine law and that, consequently, any natural law doctrine of inherent human rights is false.<sup>64</sup> These Burkean principles were no more fraudulent when Thomas R. Dew or William Harper invoked them to defend plantation slavery than when Adam Müller or Carl Ernst Jarcke invoked them to defend the privileges of *Rittergut* owners. Although an explanation of the ways in which proslavery and Old Prussian ideologues elaborated upon Burkean thought is not feasible here, an examination of their use of Burkean principles with respect to inherent human rights is instructive. Scholars on both sides of the Atlantic have suggested that the basic characteristic of all nineteenth-century conservative thought in the West was its opposition to the Enlightenment doctrine of inherent and inalienable human rights.<sup>65</sup>

Antebellum planters, apart from a small number of consistently anti-democratic ideologues like Abel P. Upshur of Virginia, talked publicly in biracial terms about the virtues of popular sovereignty and democratic republicanism for whites and the benefits of patriarchal servitude for blacks. Because proslavery ideologues by and large denied the validity of natural rights political theory only with respect to the Negro race, the proslavery argument against natural rights was burdened with ideological convolutions that had no parallel in Old Prussian thought. For *Vormärz* Junkers sovereignty resided in their Hohenzollern monarch, even though he was expected to rule in many matters with the advice and cooperation of the county and provincial assemblies dominated by *Rittergut* owners. Without a viable democratic or parliamentary tradition in Prussia prior to 1848, Old Prussian conservatives associated natural law and social contract theories—and the constitutional, representative forms of government that these ideas had spawned—with the subversive foreign influences of Jean Jacques Rousseau and the French Revolutions of 1789 and 1830.<sup>66</sup> Proslavery conservatives were faced with the more ticklish task of discrediting, or at least qualifying, the natural rights principles embodied in their own Declaration of Independence from England, written by a Virginia planter who decried the injustice of holding men as property and who hoped that both slavery and Negroes might someday be expunged from America. As Edmund Ruffin of Virginia noted in October 1858, Jefferson “has done great harm, by the countenance which his opinions on this subject, & even the words of the Declaration

<sup>64</sup> This summary is based on my own reading of Burke’s *Reflections on the Revolution in France*. Also see Herbert Dinkel, “Die konservativ Bewegung in Deutschland,” in Friedrich Schultes, ed., *Geschichte* (Frankfurt a/M., 1973), 125.

<sup>65</sup> Mannheim, “Conservative Thought,” 174; and Carl L. Becker, *The Declaration of Independence: A Study in the History of Political Ideas* (New York, 1942), 255–58.

<sup>66</sup> See, for example, Carl Ernst Jarcke, “Revolution und Absolutismus,” in his *Vermischte Schriften*, 3 vols. (Munich, 1839), 1: 174–75. This essay was originally published in 1833 in the *Berliner Politisches Wochenblatt*, founded by Old Prussian conservatives in 1831 in response to the Revolution of 1830 in Paris. Jarcke was the periodical’s first editor, 1831–32.



of Independence, have afforded to the anti-slavery fanatics of the present times." Some proslavery ideologues insisted that the slaveholding author of the Declaration of Independence had simply neglected to make the obvious distinction between the unbounded rights and freedoms possible in a state of nature and the social restraints and obligations incumbent upon any individual living in an organized community. To Jefferson, wrote William Gilmore Simms of South Carolina, "the distinction was clear between the conventional or political and the natural rights of man."<sup>67</sup> Since Jefferson's will manumitted only five of his nearly two hundred and seventy slaves,<sup>68</sup> the argument was eminently plausible. This tactic was, moreover, logically compatible with the conviction that the Negro was inherently, racially inferior to his Caucasian master, a belief that Jefferson seems to have shared. Since blacks were inferior, they could never intermingle with whites on the basis of civil, political, or social equality without causing great harm to both races. Consequently, severe restraints and obligations—slavery—had to be imposed on blacks so that they could live in white society.

Another proslavery tack, a different course to the same end, simply reconstituted the concept of natural law inherited from the Enlightenment, making white enslavement of blacks compatible with the "law of Nature." University of Virginia law professor James Philemon Holcombe maintained, "African Slavery in the United States is consistent with Natural Law, because if all the bonds of public authority were suddenly dissolved, and the community called upon to reconstruct its social and political system, the relations of the two races remaining in other respects unaltered, it would be our right and duty to reduce the negro to subjection."<sup>69</sup> This reformulation of "natural law," justifying the reduction of a portion of the community to "subjection," found striking correspondence in the thought of Carl Ludwig von Haller, a Swiss-German patrician whose political philosophy exercised a pervasive influence on Old Prussian ideologues. Contrary to the suppositions of social contract thinkers, wrote Haller in his multi-volume treatise on the "restoration" of political science begun in 1816, men have never left the state of nature, which is "the eternal, unchanging order of God" by which "the more powerful person rules, must rule, and will always rule." All social relations, said Haller, are reciprocal relationships of authority and dependence: to the strong and independent belong "authority and freedom," to the weak and needy "a greater or lesser degree of dependence or servitude." Haller's appeal to Prussian Junkers was further enhanced by his notion that every landed estate constituted a small "patrimonial state," whose proprietor wielded a degree of monarchical and protective authority over his loyal dependents comparable to that of a prince over his subjects.<sup>70</sup> But Haller and his

<sup>67</sup> Ruffin, *The Diary of Edmund Ruffin*, ed. William Kauffman Scarborough, 1 (Baton Rouge, 1972): 238; and [Simms (?)] "Thoughts on Slavery," *Southern Literary Messenger*, 4 (1838): 742.

<sup>68</sup> William Cohen, "Thomas Jefferson and the Problem of Slavery," *Journal of American History*, 56 (1969): 519.

<sup>69</sup> Holcombe, *An Address Delivered Before the Seventh Annual Meeting of the Virginia Agricultural Society, November 4th, 1858* (Richmond, Va., 1858), 4. For a broad analysis of the rejection of natural rights theory in proslavery thought, see Jenkins, *Pro-Slavery Thought in the Old South*, 125–40.

<sup>70</sup> Haller, *Restauration der Staats-Wissenschaft oder Theorie des natürlich-geselligen Zustands*, 6 vols. (2d ed., Winterthur, 1820–25), 1: 375, 340–41, 351, 6: 561–62, 2: 13–17, 57–60. Carl Ernst Jarcke's essay on the state of nature,

Old Prussian disciples, unlike proslavery conservatives in the South, had no cause to translate the distinction between the strong and the weak, the estate owner and his laborers, into a racial dichotomy.

THE HISTORICAL DEVELOPMENT AND CHARACTER of proslavery conservatism in the South and its Old Prussian counterpart in East Elbia cannot be properly understood apart from a fluctuating tension within planter and Junker ranks between what can be termed pragmatic (that is, interest-oriented and realistic) and idealistic (that is, metaphysical and romantic) defenses of the status quo. To some extent, of course, this tension reflected an apparently universal characteristic of human nature, in that the same individual is frequently torn between principled and opportunistic motives. At the same time, human nature seems to insist in the long run upon an inner reconciliation between principle and practice, in the interests of a peaceful conscience. For many planters and Junkers this reconciliation was achieved through religion. The idea that the hierarchical and authoritarian relations on the plantation or *Rittergut* were a divinely sanctioned arrangement, ordained by God to regulate the affairs of sinful men, was fundamental to the conservative arguments advanced by religious apologists for Southern and East Elbian society.<sup>71</sup> Such apologists saw relations on the plantation or *Rittergut* as having been instituted by God to assure harmonious, mutually beneficial interaction between men endowed with unequal mental and physical capacities—between the weak and the strong. Again, there was no reason in East Elbia to define the weak and the strong in racial terms, at least prior to the Junkers' growing reliance on seasonal labor from Poland during the last quarter of the nineteenth century.

The argument for divine approval of relations on the plantation or *Rittergut* struck a responsive chord in many a planter and Junker heart not merely because the gentleman farmer thought it could be an effective agent of social control when preached to his laborers but also because it fit logically and comfortably into the regional styles of pietistic Protestantism that won so many converts among the two landed elites during the first third of the nineteenth century. Both Southern evangelical Protestantism and Lutheran Neo-Pietism stressed the doctrine of original sin, thereby accentuating the inherent imperfection and inadequacy of the human condition, whether individual or collective. In so doing, these faiths denigrated the optimistic notion that man could achieve self-fulfillment through the rational manipulation of his social and physical environment. While pietistic Protestantism in the South and East Elbia counseled benevolent, humane treatment of slaves and estate laborers, its theological focus was on

which was originally published in the *Berliner Politisches Wochenblatt* in 1836, is a clear statement of Haller's ideas; see Jarcke, "Über die Entstehung des Staats durch die Natur," in his *Vermischte Schriften*, 3: 32–64.

<sup>71</sup> For representative "conservative" sermons, see Benjamin M. Palmer, "Thanksgiving Sermon, Delivered in the First Presbyterian Church, New Orleans, on Thursday, November 29, 1860," in B. M. Palmer and W. T. Leacock, *The Rights of the South Defended in the Pulpits* (Mobile, 1860), 1–11; and C. W. Hoffmann, *Predigt über Jerm. 18. 7–14. am 18. März 1849 in der evangelische Kirche in Gross-Strehlitz gehalten und dem vaterländischen Vereine hiesigen Ortes auf Verlangen zum Druck überlassen von Pastor C. W. Hoffmann* (Gross-Strehlitz, 1849).

transforming the individual's personal relation to God in heaven, not on restructuring society for the promotion of the Kingdom of God on earth.<sup>72</sup>

The tension in planter and Junker ranks between pragmatic and idealistic conservatism was not completely resolved by religion, however, because this tension was in part a manifestation of the divergence of the visionary designs of most conservative intellectuals from the more mundane concerns of most planters and Junkers.<sup>73</sup> In fact, relatively few of these agrarian entrepreneurs were much interested in the formulation of systematic philosophical defenses of their own positions. Their predominantly rural or military experiences did not dispose or equip them to evaluate their interests and values in theoretical categories; and, as owners of large estates with sizable work forces, they did not find ideological subtlety to be a necessary virtue or systematic ethics a pressing concern. The small but vocal contingent of idealistic apologists comprised men of ideas whose temperament or education inclined them to attach great significance to a degree of moral rigor and philosophical consistency that (as their own lives so often testified) is far more easily attained on the printed page than in personal or public affairs. To these intellectuals the success of the plantation or *Rittergut* as an agricultural business was less important than its embodiment of the ideal of a patriarchal and Christian community where social relations manifested less exploitation and more humanity than in the depersonalized world of urban commerce and industry. Their perceptions of the world were molded more by abstract ethical principles than by the considerations of economic and political feasibility that impelled most planters and Junkers.

Relations between idealistic intellectuals and the pragmatic majority of planters and Junkers were, as might be expected, ambiguous. On the one hand, the intellectuals tended to "romanticize" established institutions: their musings elevated life on the plantation and *Rittergut* to a noncommercial, paternalistic, and even religious level of meaning and quality. Since most people prefer to see their own behavior and motives cast in the best possible light, quite naturally pragmatic, nonspeculative planters and Junkers were at times disposed to esteem the writings and ideas of ideologues like William Gilmore Simms or Hermann Wagener, Nathaniel Beverly Tucker or Ludwig von Gerlach. On the

<sup>72</sup> On pietistic Protestantism in the South, see John R. Boles, *The Great Revival, 1787-1805: The Origins of the Southern Evangelical Mind* (Lexington, Ky., 1972), esp. 140-42; and Donald G. Mathews, *Religion in the Old South* (Chicago, 1977), esp. 62-80. On Lutheran Neo-Pietism in East Elbia, see Neumann, *Die Stufen des preussischen Konservatismus*, 87-88, 109-10; and Bigler, *The Politics of German Protestantism*, 46-50, 125-55.

<sup>73</sup> For valuable insights into the relations of idealistic proslavery ideologues and pragmatic planters, see John R. Welsh, "William Gilmore Simms, Critic of the Old South," *Journal of Southern History*, 26 (1960): 201-14; David Donald, "The Proslavery Argument Reconsidered," *ibid.*, 38 (1971): esp. 12-17; Drew Gilpin Faust, *A Sacred Circle: The Dilemma of the Intellectual in the Old South, 1840-1860* (Baltimore, 1977), esp. 59, 107-31, 147-48; and Robert J. Brugger, *Beverly Tucker: Heart over Head in the Old South* (Baltimore, 1978), esp. 107-10, 198-209. For comparably valuable insights into the relations between idealistic Old Prussian ideologues and pragmatic Junkers, see Erich Jordan, *Die Entstehung der konservativen Partei und die preussischen Agrarverhältnisse von 1848* (Munich, 1914), esp. 254-74; Mannheim, "Conservative Thought," 183-86; Neumann, *Die Stufen des preussischen Konservatismus*, esp. 54, 66, 111; and Hans-Joachim Schoeps, *Das andere Preussen: Konservative Gestalten und Probleme im Zeitalter Friedrich Wilhelms IV* (3d ed., Berlin, 1964), esp. 54-62. There were, of course, idealistic intellectuals in both the South and East Elbia who were also efficient, even innovative, owners of plantations and *Rittergüter*: Edmund Ruffin and James Henry Hammond in the South and Ludwig von der Marwitz and Ernst von Senfft-Pilsach in Prussia, to name but a few.

other hand, the intellectuals frequently reproached more mundane planters and Junkers for their failure to live up to the ideal standards of behavior set for them. The idealists, in defending plantation slavery and the Junkers' *Rittergut*-based privileges as an abstract good, were also waging an ideological crusade to purge the economic and political life of the South and East Elbia of selfish expediency, moral corruption, and intellectual vapidness.

But the distinction between idealistic intellectuals and pragmatic gentlemen farmers was more blurred in the South than in East Elbia. The planter class included a higher percentage of educated professionals—and of lawyers in particular—than did landed Junkerdom. The planters' long participation in competitive electoral politics, unlike the Junkers' experience with monarchical, military, and bureaucratic absolutism, meant that the planters were more articulate public spokesmen for their own values and interests. At the same time, antebellum party politics was less conducive to maintaining ideological purity and consistency than was the nonparliamentary, authoritarian political environment of *Vormärz* Prussia.<sup>74</sup> Most important, in defending Negro slavery before the Southern electorate, the planters could assume a simpler and more popular conservative stance than could Junkers in defending their more complex and less popular network of aristocratic privileges. For these and other reasons, the pragmatic-idealistic tension had a less dramatic and divisive impact on the history of the Old South than on the history of *Vormärz* Prussia, as the planters' behavior during the secession crisis of 1860–61 and the Junkers' behavior during the revolutionary crises of 1848–49 demonstrate.

The tension within Junker ranks between pragmatic and idealistic modes of conservatism first became apparent during the Stein-Hardenberg revolution from above. In the province of Brandenburg Karl Reichsgraf Finck von Finckenstein and Ludwig von der Marwitz (aided by the intellectual talents of Adam Müller) were opposed in principle to the reforms, while most of their peers pursued an adaptable conservative policy shaped primarily by considerations of economic self-interest.<sup>75</sup> Although the pragmatic-idealistic tension subsided with the attenuation of the reform movement during and after the Wars of Liberation against Napoleon, and remained relatively subdued for most of *Vormärz*, it exploded into open conflict during the revolutions of 1848–49 “from below.” During the spring and summer of 1848, as the confused and indecisive Frederick William IV attempted to conciliate the forces of revolution, the Junkers were temporarily shorn of absolutist protection; at the same time they confronted radical demands from the first democratically elected state assembly in Prussia's history.<sup>76</sup> The idealistic impulse in Junker conservatism was represented by a

<sup>74</sup> For suggestions to this effect, see David M. Potter, *The Impending Crisis, 1848–1861*, ed. Don E. Fehrenbacher (New York, 1976), 226; and Mannheim, “Conservative Thought,” 138–39.

<sup>75</sup> See, in particular, Gerhard Ramlow, *Ludwig von der Marwitz und die Anfänge konservativer Politik und Staatsanschauung in Preussen*, Historische Studien, no. 190 (Berlin, 1930), 78–80.

<sup>76</sup> The inner conflict between pragmatic and idealistic conservatism abated somewhat after Frederick William IV finally stiffened in November 1848, relying primarily on the strength and loyalty of the Prussian army to inaugurate a decade of political reaction. The tension remained in the background until Bismarck's *realpolitisch* and democratic methods of unifying Germany between 1866 and 1871—for example, his war against

pietistic-legitimist circle centered in the "Christian-Germanic" court clique (*Kamarilla*) close to the king; its principal spokesman was the jurist Ernst Ludwig von Gerlach. Using the new conservative newspaper *Kreuzzeitung* as a forum, Gerlach and his associates insisted that landed Junkerdom must retain a legally privileged position in state and society as the divinely ordained intermediary power between the monarch and his people. This point of view did not correspond to the outlook of the pragmatic Junkers who dominated the "General Assembly for the Protection of the Interests of Landed Property and for the Promotion of the Welfare of All Classes of People," or "Junker Parliament," which met in Berlin in August to protest the "expropriatory" agrarian reforms (including an end to the tax-exempt status of *Rittergüter* in several provinces) proposed by the Camphausen-Hansemann ministry. Gerlach attended the meetings, and he argued eloquently that *Rittergut* owners must not give up their patrimonial rights of local government, because these privileges were obligations imposed on the landed aristocracy by God. But the great majority of the two to three hundred Junkers who sat in the Junker Parliament were ready to sacrifice aristocratic privileges like patrimonial justice and police power and were willing to commute the remaining "feudal" services of lesser peasants into long-term monetary payments, if such concessions would serve to defuse popular unrest and radicalism. These men, concerned primarily about the security of their property rights, were prepared to accept the political revolution if a social revolution could be averted—averted in part by appealing to the fears of other property owners (such as well-to-do peasants and the urban middle classes) about the possibility of socialist expropriation. The septuagenarian pragmatist Ernst von Bülow-Cummerow concluded his opening address to the Junker Parliament by pointing out, "Material interests have an importance that outweighs all others; insofar as we follow them, we will always have firm ground under our feet. Let us call forth all our powers to further them, and, if we succeed, we can count on the concurrence of the great mass of people and of all propertied classes."<sup>77</sup>

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Austria and his sponsoring of a democratic suffrage for elections to the Reichstag of the North German Confederation in order to win popular support throughout Germany for a Prussian solution to the German problem—outraged the legitimist and corporatist principles of a few die-hard Old Prussian idealists like Ludwig von Gerlach and Hans Hugo von Kleist-Retzow. See Gerhard Ritter, *Die preussischen Konservativen und Bismarcks deutsche Politik, 1858–1876* (Heidelberg, 1913), 184–89, 220–21, 246–50; and Otto Pflanze, *Bismarck and the Development of Germany: The Period of Unification, 1815–1871* (Princeton, 1963), 331–34. But in response to Bismarck's proposed reforms of county government—the *Kreisordnung* of 1872, which threatened to undermine the *Rittergut* owner's traditional control over local government through a "mixture of self-government from below and bureaucratic control from above"—idealistic conviction and pragmatic self-interest coincided among Junkers more closely than they had since the Wars of Liberation against Napoleon; Robert M. Berdahl, "Conservative Politics and Aristocratic Landholders in Bismarckian Germany," *Journal of Modern History*, 44 (1972): 7–14.

<sup>77</sup> Both Gerlach's and Bülow-Cummerow's speeches were originally published in the *Kreuzzeitung* (*Neue Preussische Zeitung*), no. 44 (August 20, 1848), and no. 45 (August 22, 1848), *Beilagen*. Gerlach's speech is reprinted in Gerlach, *Aufzeichnungen aus seinem Leben und Wirken, 1795–1877*, ed. Jakob von Gerlach, 1 (Schwerin, 1903): 540–41, and in Hellmut Diwald, ed., *Von der Revolution zum Norddeutschen Bund: Aus dem Nachlass von Ernst Ludwig von Gerlach*, 1 (Göttingen, 1970): 53–54. Bülow-Cummerow's speech is reprinted in Hans Fenske, ed., *Vormärz und Revolution, 1840–1849* (Darmstadt, 1976), 332–40. For an excellent account of the Junker Parliament, see Klaus Klatte, "Die Anfänge des Agrarkapitalismus und der preussische Konservatismus" (D.Phil. dissertation, Hamburg University, 1974), 247–72.



The intensity of Southern proslavery conservatism generally reflected the intensity of the North-South sectional conflict that spawned it;<sup>78</sup> and, aside from a brief lull immediately after the Compromise of 1850, the intensity of both increased from the annexation of Texas in 1845 to the Civil War. But the idealistic-pragmatic tension in the proslavery movement never developed into a major rift. Idealists and pragmatists never disagreed over the fundamental need to maintain the institution of slavery as a rigid system of labor and social control, while in East Elbia principled and opportunistic conservatives parted company in 1848 over the question of preserving the formal legal authority of a Junker over his work force.

What conditions and circumstances present in the South and absent from East Elbia prevented the pragmatic majority of planters from considering a substantive change in their peculiar institution prior to the Civil War? One such condition was demographic. In eastern Prussia the rural population had increased since the mid-eighteenth century, particularly after 1815. By the 1830s and 1840s the region had a labor surplus. Although later drained away both by migration to the industrial cities west of the Elbe and by emigration overseas, that surplus temporarily gave *Vormärz* Junkers an abundance of cheap labor.<sup>79</sup> In the more sparsely settled South the demand for plantation labor, especially on the southwestern frontier, almost always exceeded the supply; and, without the control over their work force provided by chattel slavery, planters could not be sure of a sufficient supply of suitable gang labor. Black slaves also represented, in addition to a work force, a huge capital investment that the Junkers never had to make: slaveholders who merely kept their chattel alive during the 1850s made a respectable profit from capital gains alone. Planters were understandably concerned, therefore, about the threat to confidence in the future of slavery posed by the prospect of interference from a federal government dominated by the antislavery North.<sup>80</sup> Perhaps most important, antebellum planters refused to consider seriously even a gradual modification of slavery because of their deep-seated racial fears—manifested with painful clarity in their diatribes against abolitionism—about the general societal consequences of losing their virtually absolute legal control over several million Negro slaves. Howell Cobb, a prominent cotton planter from Houston County, Georgia, virulently attacked the alleged African descendants of sinful Ham in *A Scriptural Examination of the Institution of Slavery* (1856). After acknowledging that some “hard masters” committed illegal “excesses” in punishing their slaves, Cobb maintained, “We have to control a race of human beings who are under the influence of the most de-

<sup>78</sup> A notable exception to this rule was Thomas R. Dew’s “Review of the Debate in the Virginia Legislature of 1831–’32,” *Political Register*, 2 (1833): 769–831, reprinted in *The Pro-Slavery Argument, as Maintained by the Most Distinguished Writers of the Southern States* (Charleston, S.C., 1852). Dew, a professor at, and later president of, William and Mary College, wrote this treatise in 1832 in response to proposals introduced in the Virginia legislature of 1831–32 for the gradual emancipation and removal of Virginia’s slaves; these proposals were, in turn, a response to the Nat Turner slave revolt in Southampton County during August 1831, which many Southerners attempted to blame on abolitionist agitation in the North.

<sup>79</sup> Schissler, *Preussische Agrarverhältnisse im Wandel*, 162–63, 198.

<sup>80</sup> Wright, *The Political Economy of the Cotton South*, 141–50. Wright has overstated his case, however, by explaining secession solely in economic terms and, thereby, implicitly downgrading the racial issues that concerned slaveholders and nonslaveholders alike.



praved and vicious propensities that ever marked the character of the debased individuals of which race seem to be incapable of redemption, either by kindness or severity."<sup>81</sup>

In short, pragmatic planters and idealistic intellectuals were united by their apprehensions for the security of slave property and by their profound racial fears about the consequences of emancipation—fears that magnified their terror at the thought of a retributive slave rebellion. These concerns, moreover, were those most likely to unite the white South in support of secession. In 1860–61, when planter and proslavery interests in eleven states concluded that the election and conduct of a “Black Republican” president posed a real and present danger to the stability, even the survival, of slavery, pragmatic and idealistic conservatives agreed that secession was the necessary, if regrettable, step to be taken—that a political revolution was necessary to avert a social one. William Henry Holcombe, a well-educated and reflective slaveholding physician in Louisiana, maintained that Lincoln’s victory offered Southerners “the alternative” of secession and “a separate nationality” or “the Africanization of the South” within the federal Union.<sup>82</sup> A more materialistic perspective characterized the official “Declaration” of the reasons for secession published by the Mississippi state convention: “We must either submit to degradation, and to the loss of property worth four billions of money, or we must secede from the Union framed by our fathers, to secure this as well as every other species of property.”<sup>83</sup>

WHAT DOES A COMPARISON OF ANTEBELLUM PLANTERS and *Vormärz* Junkers reveal about the “conservative” qualities of the planter class and proslavery thought? If, following Burke, respect for established property rights and regard for existing civil, political, and social inequalities are posited as archetypal conservative principles, then planters and proslavery ideology were perhaps even more conservative than were Junkers and Old Prussian ideology. But this overly simplistic perspective has two major shortcomings. First, it risks falling into the trap of an inflexible, homogenizing mode of social class analysis that is insensitive to the tension within both planter and Junker ranks between idealistic and pragmatic manifestations of conservatism. Even though, for a variety of historical

<sup>81</sup> Cobb, *A Scriptural Examination of the Institution of Slavery in the United States, with Its Objects and Purposes* ([Perry] Ga., 1856), 121. This Howell Cobb, who became president of the Cotton Planters’ Convention in Georgia in 1859–60, should not be confused with the famous Clarke County politician of the same name.

<sup>82</sup> Holcombe, *The Alternative: A Separate Nationality or the Africanization of the South?* (n.p., 1860), reprinted in the *Southern Literary Messenger*, 32 (1861): 81–88. William Henry Holcombe—brother of James P., cited in note 71, above—was born in Lynchburg, Va., and graduated from the medical department of the University of Pennsylvania in 1847. From 1855 to 1862 he resided in Tensas Parish, La., but had close ties to Natchez, Miss., society. His manuscript diary in the Southern Historical Collection, University of North Carolina, Chapel Hill, and his several poems published in the *Southern Literary Messenger* in 1860 and 1861 testify to his reflective and idealistic bent.

<sup>83</sup> *An Address Setting Forth the Declaration of the Immediate Causes Which Induce and Justify the Secession of the State of Mississippi from the Federal Union and the Ordinance of Secession* (Jackson, Miss., 1861), 5. At least eighty-five of the one hundred members of the Mississippi secession convention were slaveholders; forty-four owned twenty or more slaves, and sixty-one owned ten or more slaves; Ralph A. Wooster, *The Secession Conventions of the South* (Princeton, 1962), 32.

reasons, the pragmatic-idealistic tension had less impact on antebellum planters than on contemporary Junkers, the historian of the Old South should nevertheless be cognizant of a divergence between the transcendent, romantic values and ideas that inspired a good many proslavery ideologues and the more mundane concerns and practical attitudes that characterized the majority of planters.

Second, reliance on Burkean principles as the touchstone of nineteenth-century conservatism in the West serves to reinforce the one-sided emphasis in both American and German historiography on the structure and content of proslavery or Old Prussian ideology as the measure of authentic conservatism among planters and Junkers. Historically, conservatism has been a political as well as an intellectual phenomenon; and to study ideological conservatism apart from its complex interaction with political conservatism is to present a partial, even distorted, view of the past. Political scientist Hans-Gerd Schumann has persuasively argued that "the real criterion of conservatism" is "the conscious political action taken by socially privileged classes, strata, or groups to safeguard the institutions in which their social position is embedded against attempts to alter the norms prevailing in the political domain, or polis."<sup>84</sup> Accordingly, the true measure of planter or Junker conservatism is to be found in the assiduousness with which each landed elite fought in the political arena to protect its plantation- or *Rittergut*-centered world. Perhaps Southern planters should again be adjudged more conservative than *Vormärz* Junkers, because they had a greater compulsion in 1860–61 to preserve the traditional character of the plantation as a hierarchical and authoritarian private law state than did most Junkers in 1848–49 with regard to their *Rittergüter*. The Junkers could afford to sacrifice some of the traditional character of the *Rittergut* as a private law state, since, unlike planters, they were privileged members of a larger hierarchical and authoritarian political order. Political flexibility, even more than the weakness and divisiveness of their foes, gave the Junkers a durability that history denied to the planters, whose inflexibility grew as their political influence over the federal government waned, until they finally determined to wager their survival on secession and civil war. Not only did the planters face stronger and more intractable opposition than did the Junkers, but the financial and racial characteristics of Southern slavery also engendered in planters far greater fears about the economic and social consequences of losing personal control over their labor force.

<sup>84</sup> Schumann, "The Problem of Conservatism: Some Notes on Methodology," 807.

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## In Defense of Servitude: American Proslavery and Russian Proserfdom Arguments, 1760–1860

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PETER KOLCHIN

CONSERVATIVE IDEOLOGIES INSPIRE little interest today. A generation believing in the axiomatic virtues of freedom and equality—however they may be defined—finds little to celebrate in men who defended social systems predicated on inequality and the restriction of the freedom of a substantial proportion of the population. It is not surprising, therefore, that modern historians, while dissecting the lives, thoughts, and motives of abolitionists, have all but ignored the defenders of human bondage. Even historians of such bondage—whether in the United States South,<sup>1</sup> Russia,<sup>2</sup> or Latin America—have paid relatively little attention to the proponents of unfree labor and have commonly assumed in passing that proslavery and proserfdom arguments consisted largely of self-serving rhetoric unworthy of being taken seriously.

This article, based on research I have been conducting for a general comparison of American slavery and Russian serfdom, is a revised version of a paper presented at the annual meeting of the American Historical Association in December 1978. In making revisions I have benefited from helpful criticisms by Daniel Field, George M. Fredrickson, and two anonymous referees for the *American Historical Review*.

<sup>1</sup> The only comprehensive survey of American proslavery thought remains William Sumner Jenkins's *Pro-Slavery Thought in the Old South* (Chapel Hill, N.C., 1935). For important recent exceptions to the general neglect of the subject, see Eugene D. Genovese, *The World the Slaveholders Made: Two Essays in Interpretation* (New York, 1969); George M. Fredrickson, *The Black Image in the White Mind: The Debate on Afro-American Character and Destiny, 1817–1914* (New York, 1971), chaps. 2, 3; Larry Robert Morrison, "The Proslavery Argument in the Early Republic, 1790–1830" (Ph.D. dissertation, University of Virginia, 1975); and Larry Edward Tise, "Proslavery Ideology: A Social and Intellectual History of the Defense of Slavery in America, 1790–1840" (Ph.D. dissertation, University of North Carolina, 1975). The following works deal with particular aspects of the defense of slavery: David Donald, "The Proslavery Argument Reconsidered," *Journal of Southern History* [hereafter, *JSH*], 37 (1971): 3–18; Drew Gilpin Faust, *A Sacred Circle: The Dilemma of the Intellectual in the Old South, 1840–1860* (Baltimore, 1977), 113–31; Ralph E. Morrow, "The Proslavery Argument Revisited," *Mississippi Valley Historical Review*, 48 (1961): 79–94; H. Shelton Smith, *In His Image, But . . . : Racism in Southern Religion, 1780–1910* (Durham, N.C., 1972), 129–64; William Stanton, *The Leopard's Spots: Scientific Attitudes toward Race in America, 1815–59* (Chicago, 1960); and Ronald T. Takaki, *A Pro-Slavery Crusade: The Agitation to Reopen the African Slave Trade* (New York, 1971).

<sup>2</sup> Russian historians have paid even less attention to the defense of serfdom than American historians have devoted to the defense of slavery. Most Russian writing on the subject appears in passing, in works primarily devoted to the opposition to serfdom. See V. I. Semevskii, *Krest'ianskii vopros v Rossii v XVIII i pervoi polovine XIX veka*, 2 vols. (St. Petersburg, 1888); M. T. Beliaevskii, *Krest'ianskii vopros v Rossii nakanune vosstaniia E. I. Pugacheva (formirovanie antikrepostnicheskoi mysli)* (Moscow, 1965); and V. V. Mavrodin, *Klassovaia bor'ba i obshchestvenno-politicheskaia mysl' v Rossii v XVIII v. (1725–1773 gg.)* (Leningrad, 1964). For an exception that focuses on the life of an important ideologue, see I. A. Fedosov, *Iz istorii russkoi obshchestvennoi mysli XVIII stoletia: M. M. Shcherbatov* (Moscow, 1967).

This assumption, and the ensuing neglect, are unfortunate for several reasons. First, they are based on the ahistorical judgment that people living in the past must share our values, that no one could *really* oppose such obviously supportable concepts as universal freedom and equality. Second, the defense of bondage is intrinsically interesting, especially in the United States, where proslavery spokesmen expounded the most imaginative—one scholar has suggested the only—conservative philosophy in the nation's history.<sup>3</sup> But, most important, an examination of the arguments used to defend unfree labor can provide information about the masters and even about the labor systems they directed. Only by taking seriously their ideology can we take seriously their history.

This article examines two such conservative ideologies. The first half of the essay suggests a basic similarity of arguments used to defend unfree labor in Russia and the United States, and the second points to an important difference in the development of these arguments over time. Both the similarity and the difference have valuable implications.

IN 1859 BRITISH TRAVELER Charles Henry Pearson noted a widespread belief among Russian noblemen that the peasants "are not fit for freedom." Such sentiment puzzled him since, as he put it, "there is no difference of race between the governors and the governed."<sup>4</sup> Pearson had made an observation that implicitly raised an important issue. Russian serfdom, unlike American slavery, was a system of bondage that was not based on any racial distinction between master and laborer. Surely, one might postulate, this difference would be reflected in the arguments used to defend the two institutions.

Certainly, racial arguments in defense of slavery were pervasive in the South. They were common, of course, among "scientific" racists and ethnologists such as Dr. Samuel A. Cartwright, who concluded that blacks, with their smaller brains, sloping foreheads, and deficient respiratory systems, were physiologically so different from whites that they were fit only for slavery.<sup>5</sup> But racial arguments were also common among Southerners who defended slavery on other grounds. They were present early, as in eighteenth-century planter Landon Carter's casual remark in his diary that blacks "are devils and to make them otherwise than slaves will be to set devils free," and late, as in the insistence of James H. Hammond of South Carolina that emancipation was impossible because "the doom of Ham has been branded on the form and features of his African descendants."<sup>6</sup> Throughout the South, whites insisted that blacks were different, inferior, and suited for slavery.<sup>7</sup>

<sup>3</sup> See Louis Hartz, *The Liberal Tradition in America: An Interpretation of American Political Thought since the Revolution* (New York, 1955), pt. 4, esp. 174, 176.

<sup>4</sup> Pearson, *Russia, by a Recent Traveller: A Series of Letters* (1859; reprint ed., London, 1970), 22–23.

<sup>5</sup> See Cartwright, "Slavery in the Light of Ethnology," in E. N. Elliott, ed., *Cotton Is King, and Pro-Slavery Arguments* (1860; reprint ed., New York, 1970), 689–727. On scientific racism, see Stanton, *The Leopard's Spots*; and Fredrickson, *The Black Image in the White Mind*, chaps. 2, 3.

<sup>6</sup> Carter, *The Diary of Colonel Landon Carter of Sabine Hall, 1752–1778*, ed. Jack P. Greene, 2 vols. (Charlottesville, Va., 1965), 2: 1148–49; and Hammond, "Speech on the Justice of Receiving Petitions for the Abolition of Slavery in the District of Columbia," in Hammond, *Selections from the Letters and Speeches of the Hon. James H. Hammond, of South Carolina* (New York, 1866), 37–38.

<sup>7</sup> On Southern racial consciousness, especially see Winthrop D. Jordan, *White over Black: American Attitudes*

What is striking, however, is that similar arguments were used to defend serfdom, even though master and serf were usually of the same national and racial origin.<sup>8</sup> By the eighteenth century, Russian noblemen had come to regard themselves as inherently different from their peasants and the serfs as inherently incapable of freedom.<sup>9</sup> This sense of distinction between lord and peasant deepened over the course of the eighteenth century; Peter I contributed to it by forcing noblemen to shave their beards and adopt Western European manners, but equally important was the natural cultural division between two very different social classes. In the hundred years before emancipation, nobleman and peasant inhabited such different worlds that the distinction between the two seemed as inherent—as “racial”—as the distinction between white and black.<sup>10</sup>

It is noteworthy, then, but perhaps not entirely surprising, that Russian noblemen invented many of the same kinds of racial arguments to defend serfdom that American slaveowners used to justify their peculiar institution. The arguments were not as elaborately worked out: one does not find tomes exploring the physiological differences between peasant and nobleman (although in the eighteenth century some noble spokesmen claimed that, whereas they had white bones, peasants had black bones<sup>11</sup>). The basic assumptions, however, were similar; they were “racial” in that they were predicated on belief in inherent and immutable differences rather than in distinctions based on particular social or environmental conditions. Peasants were just as intrinsically lazy, childlike, and requiring of direction as were blacks. Thus, Prince M. M. Shcherbatov, the most persistent exponent of the nobility’s interests in the late eighteenth century, asserted that without strict supervision peasants would not perform agricultural labor because of their “voluptuousness” and “laziness.”<sup>12</sup> In 1802 author Nicholas M. Karamzin rebutted the environmentalist argument, sometimes advanced by foreigners, that Russian peasants were lazy *because* of serfdom; on the contrary, he insisted, “they are lazy from nature, from habit, from ignorance of the advantages of diligence.”<sup>13</sup> Just as defenders of American slavery saw free

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toward the Negro, 1550–1812 (1968; reprint ed., Baltimore, 1969); and Fredrickson, *The Black Image in the White Mind*, chaps. 2, 3.

<sup>8</sup> The lack of racial or national distinction between lord and peasant makes the comparison of Russian serfdom with American slavery especially fruitful and flies in the face of assumptions by some scholars that enslavement of one’s own people is virtually impossible. “What sets the slave apart from all other forms of involuntary labor is that, in the strictest sense, he is an outsider,” Moses I. Finley has written. “He is brought into a new society violently and traumatically; he is cut off from all traditional human ties of kin and nation and even his own religion.” Finley, “The Idea of Slavery: Critique of David Brion Davis’ *The Problem of Slavery in Western Culture*,” in Laura Foner and Eugene D. Genovese, eds., *Slavery in the New World: A Reader in Comparative History* (Englewood Cliffs, N.J., 1969), 260. This was clearly not the case, however, with Russian serfs, who, as numerous historians have pointed out, were essentially slaves. See, for example, Jerome Blum, *Lord and Peasant in Russia: From the Ninth to the Nineteenth Century* (Princeton, 1961), 468–69.

<sup>9</sup> Some foreign travelers shared this view. “There is something of the Negro in the nature of a Russian,” wrote Frenchman Germain de Lagny; see his *The Knout and the Russians: or, The Muscovite Empire, the Czar, and His People*, trans. John Bridgeman (London, 1854), 154.

<sup>10</sup> A. Romanovich-Slavatinskii, *Dvorianstvo v Rossii ot nachala XVIII veka do otmeny krepostnago prava* (St. Petersburg, 1870), 58–87; and P. K. Alefirenko, “Russkaia obshchestvennaia mysl’ pervoi poloviny XVIII stoletia o sel’skom khoziaistve,” in *Materialy po istorii zemledeliia SSSR*, 1 (Moscow, 1952): 545–47.

<sup>11</sup> Romanovich-Slavatinskii, *Dvorianstvo v Rossii*, 71.

<sup>12</sup> Shcherbatov, “Razsuzhdenie o nyneshnem v 1778 godu pochti povsemestnom golode v Rossii, o sposobakh onomu pomoch’ i vpred predupredit’ podobnoe zhe neshchastie,” in his *Sochineniia kniazia M. M. Shcherbatova*, ed. I. P. Khrushchov, 1 (St. Petersburg, 1896): 631–32.

<sup>13</sup> Karamzin, “Pis’mo sel’skogo zhitelia,” in his *Sochineniia Karamzina*, ed. Aleksandr Smirdin, 3 (St. Peters-



blacks as an anomaly and insisted that all Negroes were better off as slaves,<sup>14</sup> so too advocates of Russian serfdom pointed to their “free” (or state) peasants and maintained that they would be happier as serfs, under the protection of *pomeshchiki* (noble landowners). During his short reign (1796–1801), Emperor Paul was able to act on his belief in the desirability of distributing “all state peasants to *pomeshchiki*” by awarding generous grants of state lands and peasants to deserving noblemen; and he rejoiced in placing peasants under the supervision of “police masters,” who would both care for them and guard the peace and security of the state.<sup>15</sup>

If racial arguments would seem to be particularly suited to the defense of black slavery, then surely “class” arguments would be the pre-eminent Russian defense of serfdom, for few Western societies in the late eighteenth and early nineteenth centuries had so many hereditary, legal class distinctions as did imperial Russia. And class arguments did abound. Russian noble spokesmen eagerly composed paeans to the virtue, honor, and service of the nobility, which they contrasted with the money-grubbing ways of the merchants.<sup>16</sup> If merchants deserved scorn, peasants required care and supervision. “The principal right of a Russian nobleman is to be a *pomeshchik*,” wrote Karamzin in an essay extolling his own patriarchal regard for his “people”; “his principal duty is to be a good *pomeshchik*.”<sup>17</sup> Serfdom enabled noblemen to care for and protect their well-meaning but ignorant charges. Playwright and poet Ivan Boltin emphasized the

burg, 1848): 573. For the prevalence of such views among seigneurs of other European countries, see Jerome Blum, *The End of the Old Order in Rural Europe* (Princeton, 1978), 45–46.

<sup>14</sup> See, for example, the comments of Thornton Stringfellow: “Slavery is becoming, to this people [Negroes], so manifestly a blessing in our country that fugitives from labor are constantly returning to their masters again, after tasting the blessings, or rather the awful curse to them, of freedom in nonslaveholding States; and while I write, those who are lawfully free in this State, are praying our Legislature for a law that will allow them to become slaves.” Stringfellow, “Statistical View of Slavery,” in Elliott, *Cotton Is King*, 540. On Southern white distaste for free Negroes, see Ira Berlin, *Slaves without Masters: The Free Negro in the Antebellum South* (New York, 1974), 316–80.

<sup>15</sup> Paul I, as quoted in N. M. Druzhinin, *Gosudarstvennye krest'iane i reforma P. D. Kiseleva*, 1 (Moscow, 1946): 147. For other expressions of this viewpoint, see Shcherbatov, “Razsuzhdenie o nyneshnem v 1778 godu pochtii povsemestnom golode v Rossii,” 645–47; Karamzin, “Pis'mo sel'skago zhitelia,” 574; A. I. Komissarenko, “Proekt vvedeniia lichnoi krepostnoi zavisimosti ekonomicheskikh krest'ian v Rossii v pervye gody posle sekularizatsii tserkovnykh imushchestv (60-e gody XVIII veka),” *Ezhegodnik po agrarnoi istorii Vostochnoi Evropy 1970 g.* (1977): 95–103; and “Obozrenie raspolozheniia umov i razlichnykh chastei gosudarstvennogo upravleniia v 1835 godu,” in E. A. Morokhovets, ed., *Krest'ianskoe dvizhenie 1827–1869 godov*, 1 (Moscow, 1931): 18. On attitudes toward state peasants and on their inadequate supervision, see Druzhinin, *Gosudarstvennye krest'iane i reforma P. D. Kiseleva*, 125–47.

<sup>16</sup> The foremost noble opponent of merchant pretensions was Prince Shcherbatov, who during the 1760s, 1770s, and 1780s spent much of his public efforts clarifying the distinction between nobleman and merchant. See, for example, his “Razmyshlenie o dvorianstve,” in his *Sochineniia*, 219–68, and “Razsmotrenie o voprose—mogut li dvoriane zapisyvai'sia v kuptsy,” in his *Neizdannye sochineniia* (Moscow, 1935), 139–58. On Shcherbatov, see Fedosov, *Iz istorii russkoi obshchestvennoi mysli*; Marc Raeff, “State and Nobility in the Ideology of M. M. Shcherbatov,” *American Slavic and East European Review*, 29 (1960): 363–79; and Joan M. Afferica, “The Political and Social Thought of Prince M. M. Shcherbatov, 1733–1790” (Ph.D. dissertation, Harvard University, 1966). On noble efforts to resist merchant encroachments during the last third of the eighteenth century, see Wilson Robert Augustine, “The Economic Attitudes and Opinions Expressed by the Russian Nobility in the Great Commission of 1767” (Ph.D. dissertation, Columbia University, 1969), 81–89; Beliaevskii, *Krest'ianskii vopros v Rossii nakanune vosstaniia E. I. Pugacheva*, 87–88, 195–96, 239–43; Paul Dukes, *Catherine the Great and the Russian Nobility: A Study Based on the Materials of the Legislative Commission of 1767* (Cambridge, 1967), 113–15, 129–30; and Robert E. Jones, *The Emancipation of the Russian Nobility, 1762–1785* (Princeton, 1973), 64–67, 149–51.

<sup>17</sup> Karamzin, “Pis'mo sel'skago zhitelia,” 579.



"affection of the slaves for their masters" and noted that these masters "maintain their slaves as the duty of humanity demands"; he contrasted the happiness of the Russian peasant with the misery of the poor in Western Europe.<sup>18</sup> Count D. P. Buturlin, writing in French to his uncle in 1803, succinctly expressed the paternalistic assumptions inherent in the defense of serfdom. "There is something paternal and gentle in the reciprocal relationship between the master and his born servant, whereas this same relationship strikes me as purely mercenary when between the hired servant and his master." In the latter case, he explained, "it is a free market, an exchange of his service for my money, and from that point it seems to me that I am finished with everything when I have paid him."<sup>19</sup> Often Russians contrasted the order, patriarchal relations, and security enjoyed by their peasants with the poverty, exploitation, and insecurity of free Western Europeans.<sup>20</sup>

Such arguments also abounded, however, in the antebellum South. Indeed, a perusal of proslavery writings provides ample evidence that a paternalistic insistence on the humanity and harmony of slavery was as pervasive as the racial argument in its defense, especially during the last two decades before the Civil War. Most historians are aware of the writings of George Fitzhugh,<sup>21</sup> but perhaps the attention his works have received has obscured similar justifications developed by a host of other Southerners, such as James H. Hammond, William Harper, William Gilmore Simms, Henry Hughes, Thomas R. Dew, and Thomas R. R. Cobb. The humanity of slavery, the reciprocal relationship between master and slave, and the brutality of the free-labor system were at the core of their defense of the peculiar institution. As Baptist minister Thornton Stringfellow put it in an otherwise largely religious justification of the treatment of slaves, "Their condition, *as a class*, is now better than that of any other equal number of laborers on earth, and is daily improving."<sup>22</sup>

<sup>18</sup> Ivan Boltin, *Primechaniia na Istoriu drevniia i nyneshniia Rossii g. Leklerka*, 2 (n.p., 1788): 243–44. For similar arguments, see M. M. Shcherbatov, "Na chitannii oktiabria 12 chisla golos gospodina deputata goroda Serpeiska, Rodiona Glinkova," in his *Sochineniia*, 128–29, and "Primechanie na 13-iu stat'iu II glavy proekta pravam blagorodnykh," *ibid.*, 195–96; E. R. Dashkova, "Memoires de la Princesse Dashkawa," in *Arkhiv kniazia Vorontsova*, 40 vols. (Moscow, 1870–97), 21: 137; Karamzin, "Pis'mo sel'skago zhitelia," 579–80; and Richard Pipes, ed., *Karamzin's Memoir on Ancient and Modern Russia: A Translation and Analysis* (Cambridge, Mass., 1969), 165.

<sup>19</sup> D. P. Buturlin to his uncle, S. R. Vorontsov, St. Petersburg, July 30, 1803, in *Arkhiv kniazia Vorontsova*, 32: 366.

<sup>20</sup> See, for example, F. V. Rostopchin, "Vozrazhenie Grafa Rastopchina, na knigu, sochinennuu Grafom Stroinovskim, o usloviakh s krest'ianami" (1811), *Chteniiia v Imperatorskom obshchestve istorii i drevnostei Rossiiskikh pri Moskovskom universitete*, 30, no. 3, pt. 5 (1859): 40; and A. S. Shishkov, *Zapiski, mneniia, i perepiski Admirala A. S. Shishkova*, 2 (Berlin, 1870): 128–29. On paternalistic defenses of serfdom under Nicholas I, see S. P. Mel'gunov, "Epokha 'ofitsial'noi narodnosti' i krepostnoe pravo," in A. K. Dzhivelegov et al., eds., *Velikaia reforma: Russkoe obshchestvo i krest'ianskii vopros v proshlom i nastoiashchem. Iubileinoe izdanie*, 2 (Moscow, 1911): 8–14.

<sup>21</sup> George Fitzhugh, *Sociology for the South: or, The Failure of Free Society* (Richmond, Va., 1854), and *Cannibals All! or, Slaves without Masters* (1857), ed. C. Vann Woodward (Cambridge, Mass., 1960). On Fitzhugh, see Harvey Wish, *George Fitzhugh: Propagandist of the Old South* (Baton Rouge, 1943); C. Vann Woodward, "George Fitzhugh, *Sui Generis*," Introduction to Fitzhugh, *Cannibals All!*, vii–xxxix; and Genovese, *The World the Slaveholders Made*, pt. 2.

<sup>22</sup> Stringfellow, "The Bible Argument: or, Slavery in the Light of Divine Revelation," in Elliott, *Cotton Is King*, 491. Many of the best paternalistic defenses of slavery were reprinted in the 1850s in two large collections: *The Pro-Slavery Argument, as Maintained by the Most Distinguished Writers of the Southern States* (Charleston, S.C., 1852), and E. N. Elliott's much expanded volume, *Cotton Is King*. Leading Southern periodicals also frequently published paternalistic defenses of slavery. See, for example, W., "Slavery in the Southern States,"

Antebellum Southerners recognized that many of their best arguments in favor of slavery were nonracial, and the less timid among them fully acknowledged this fact. They noted that throughout history slavery had been a prerequisite for civilization and pointed to the slave societies of antiquity and the unfree labor systems of medieval Europe as precedents for Southern slavery.<sup>23</sup> These Southerners were not stupid men, and they realized that most of the slave systems they cited were not based on racial distinctions. The logic of their position, therefore, increasingly led them to broaden the defense of slavery to that of a superior social system, regardless of race. They extolled slavery for serving the best interests of all elements in society, without pitting class against class, while fostering all of the tried and true social virtues.<sup>24</sup> They did not deny that blacks were ideally suited for slavery, but they treated this predisposition as a fortunate accident rather than as the essential reason for slavery: had Africans not existed, other slaves would have been required in their place. As Hammond put it in his famous mud-sill speech,

In all social systems there must be a class to do the menial duties, to perform the drudgery of life. That is, a class requiring but a low order of intellect and but little skill. Its requisites are vigor, docility, and fidelity. Such a class you must have, or you would not have that other class which leads progress, civilization, and refinement. It constitutes the very mud-sill of society and of political government; and you might just as well attempt to build a house in the air, as to build either the one or the other, except on this mud-sill.<sup>25</sup>

The South was fortunate in having found blacks, but Northern society also rested upon a mud-sill of workers who were "essentially slaves."

During the 1840s and 1850s the boldest and most consistent of the proslavery advocates increasingly downplayed race as a justification for slavery and made explicit their belief that slavery was part of a superior social system, regardless of any racial differences within society. This point of view never dominated the an-

*Southern Literary Messenger*, 9 (1843): 736-44; and Solon Robinson, "Negro Slavery at the South," *DeBow's Review*, 7 (1849): 206-25, 379-89.

<sup>23</sup> See, for example, Chancellor Harper, "Slavery in the Light of Social Ethics," in Elliot, *Cotton Is King*, 549-52, 574-75, 604-06; J. H. Hammond, "Slavery in the Light of Political Science," *ibid.*, 634-37; Thomas R. Cobb, *An Inquiry into the Law of Negro Slavery in the United States of America* (Philadelphia, 1858), xxxvi-cxxi; Matthew Estes, *A Defence of Negro Slavery as It Exists in the United States* (Montgomery, Ala., 1846), 13-48; Fitzhugh, *Cannibals All!*, *passim*; and J. D. B. DeBow, "The Origin, Progress, and Prospect of Slavery," *DeBow's Review*, 9 (1850): 9-19. Occasionally, proslavery writers even used Russian serfdom as a precedent: Russian serfs, noted Cobb, "are contented with their lot and seek no change. They are indolent, constitutionally. . . . They are mendacious, beyond the negro perhaps, and feel no shame at detection. Like him, too, they have no providence for the future, and no anxiety about it." Cobb, *An Inquiry into the Law of Negro Slavery*, cxviii. On the precedent of classical slavery, see Edwin A. Miles, "The Old South and the Classical World," *North Carolina Historical Review*, 48 (1971): 258-75.

<sup>24</sup> See, for example, Hammond, "Slavery in the Light of Political Science," 643-46, and "Speech on the Admission of Kansas . . . , March 4, 1858," in Hammond, *Selections from the Letters and Speeches of the Hon. James H. Hammond*, 317-19; Thomas R. Dew, "Professor Dew on Slavery," in *The Pro-Slavery Argument*, 325-26, 457-61; Cobb, *An Inquiry into the Law of Negro Slavery*, xxxvi, ccxvii-ccxviii; D. R. Hundley, *Social Relations in Our Southern States* (New York, 1860), 64-70; [Edwin C. Holland] *A Refutation of the Calumnies against the Southern and Western States* . . . (1822; reprint ed., New York, 1969), 45-61; Henry Hughes, *Treatise on Sociology, Theoretical and Practical* (1854; reprint ed., New York, 1968), *passim*; Stringfellow, "Statistical View of Slavery," 533-34, 539; and Fitzhugh, *Cannibals All!*, *passim*. On Fitzhugh's development of this argument, see Genovese, *The World the Slaveholders Made*, 165-234.

<sup>25</sup> Hammond, "Speech on the Admission of Kansas," 318-19.

tebellum South's defense of slavery, because planters needed the political support of nonslaveholding whites, but it was not limited to Fitzhugh or to a handful of atypical eccentrics.<sup>26</sup> "Pity it is," wrote author William Gilmore Simms, "that the louzy and lounging lazzaroni of Italy cannot be made to labor in the fields under the whip of a severe task-master!"<sup>27</sup> Henry Hughes, in his highly abstract defense of "warranteeism," as he renamed slavery, noted that the "ethnical qualification" was "accidental." "Warranteeism without the ethnical qualification," he concluded, "is that to which every society of one race must progress."<sup>28</sup> In his diary, fire-eater Edmund Ruffin praised George Fitzhugh's analysis of the "slavery of labor to capital" and ridiculed the notion that "the Africans generally, or the negroes particularly, are descended from Ham."<sup>29</sup> Fitzhugh's lead article in the October 1857 issue of the influential *DeBow's Review* argued forcefully that even among whites free labor was inferior to slave labor and chided Southerners for failing to carry proslavery arguments to their logical conclusion. "Domestic slavery must be vindicated in the abstract, and in the general," he asserted, "as a normal, natural, and, *in general*, necessitous element of civilized society, without regard to race or color."<sup>30</sup>

Russians, then, developed an essentially racial argument in defense of serfdom, even though no racial distinction divided lord and peasant; at the same time, Americans elaborated an ideology that stressed the virtues of aristocracy and *noblesse oblige* in a society with democratic pretensions. Clearly, the need to defend unfree labor had a logic of its own that propelled Russians and Americans to arrive independently at almost all of the same major arguments and conclusions. The religious justification for bondage, which flourished in the United States but was perfunctory in Russia, was a major exception. *Pomeshchiki* told their serfs that their status was ordained by God and regularly relied on priests to instill obedience in their peasant parishioners, and defenders of serfdom often made passing references to a God-given order. But religious arguments were rarely central to formal writings advocating serfdom.<sup>31</sup>

In addition to racial and paternalistic arguments, two practical points received widespread circulation in both countries. The first was the economic ne-

<sup>26</sup> Drew Faust has seen South Carolina's leading proslavery spokesmen as alienated intellectuals who through "their essays on slavery won recognition other intellectual efforts failed to secure"; *A Sacred Circle*, 116, 113-31.

<sup>27</sup> Simms, "The Morals of Slavery," in *The Pro-Slavery Argument*, 265.

<sup>28</sup> Hughes, *Treatise on Sociology*, 207.

<sup>29</sup> Ruffin, *The Diary of Edmund Ruffin*, ed. William Kauffman Scarborough, 1 (Baton Rouge, 1972): 240, 308.

<sup>30</sup> Fitzhugh, "Southern Thought," *DeBow's Review*, 23 (1857): 347.

<sup>31</sup> For a description of Russian religious justifications, see Mel'gunov, "Epokha 'ofitsial'noi narodnosti,'" 12-14; and, for examples of the far more numerous and detailed American religious arguments on behalf of slavery, see Richard Furman, *Rev. Dr. Richard Furman's Exposition of the Views of the Baptists, Relative to the Coloured Population of the United States* (Charleston, S.C., 1822), 7-12, 16-17; Charles Hodge, "The Bible Argument on Slavery," in Elliott, *Cotton Is King*, 841-77; Stringfellow, "The Bible Argument"; and Albert Taylor Bledsoe, "Liberty and Slavery: or, Slavery in the Light of Moral and Practical Philosophy," in Elliott, *Cotton Is King*, esp. 337-80. Most of the other defenses of slavery, which I have cited above, mention, at least in passing, the religious justification. One reason for the prevalence of religious justifications of American slavery is doubtless the prominent role Southern clergymen played in defending the peculiar institution; see Smith, *In His Image*, 129-64; and Morrison, "The Proslavery Argument," 30-59. Whereas Protestant ministers in the United States were an independent-minded group who often spoke out on social issues, the Russian parish clergy was a closed and increasingly isolated caste, "a weak, tangential group lacking in influence and power." See Gregory L. Freeze, *The Russian Levites: Parish Clergy in the Eighteenth Century* (Cambridge, Mass., 1977), 222, 179-217.

cessity of forced labor. Americans and Russians recognized that their systems of bondage arose to meet a general labor shortage under conditions of relative population scarcity; both held out the prospect of economic disaster should unfree labor be abolished.<sup>32</sup> Even more widespread were dire predictions of social collapse—refusal to work, unrest, and rebellion—should slaves and serfs be emancipated. If Russian polemicists could point to the degenerate condition of state peasants, who were without proper seigneurial supervision, their American counterparts had a far more compelling argument in the example of emancipation in the West Indies. The British colonies of Barbados and Jamaica showed how blacks, freed from the protective care of slavery, would revert to their primitive African ways, while Haiti held out the ultimate horror of revolution. A similar nightmare stalked the Russian gentry: the bloody Pugachev rebellion of 1773–74, which for generations served as a warning to *pomeshchiki* of just how precarious their position was.<sup>33</sup>

Russians and Americans even played the same word games, insisting that their particular form of bondage was the mildest, or even that it was not really bondage at all. In the eighteenth and early nineteenth centuries, Russians commonly used the word “slavery” (*rabstvo*) to describe the status of their serfs, but defenders of serfdom often pointed out that their form of slavery was different from others. “Between freedom and freedom, and between slavery and slavery, there is a difference, and this difference is great and varied,” wrote Boltin; “a title means nothing.” “There is,” he continued, “freedom that is worse than slavery.” Naturally, Russian slavery was a variety better than most freedom.<sup>34</sup> Precisely the same kinds of assertions were made in the United States: Henry Hughes’s refusal to use the word “slavery,” for which he substituted “warrantecism,” was unusual, but many held with Hammond that slavery was but a name that meant little, for the working classes everywhere were really slaves. “The difference between us,” he lectured Northerners, “is that our slaves are hired for life and well compensated. . . . Yours are hired by the day, not cared for, and scantily compensated.” Matthew Estes agreed that the word “slavery”

<sup>32</sup> See, for example, M. M. Shcherbatov, “Zapiska po krest’ianskomu voprosu,” in his *Neizdannnye sochineniia*, 8–9; Major General Afanasii Lavrent’evich Komarov, Essay in *Trudy Vol’nago ekonomicheskago obshchestva k pooshchreniiu v Rossii zemledeliia i domostroitel’stva*, 66 (1814): 128–61; G. R. Derzhavin, *Sochineniia Derzhavina s ob’iasnitel’nymi primechaniiami Ia. Grot*, 6 (St. Petersburg, 1876): 774–75; Chancellor Harper, “Harper on Slavery,” in *The Pro-Slavery Argument*, 85–94; Estes, *A Defence of Negro Slavery*, 155–62; Francis J. Grund, *The Americans in Their Moral, Social, and Political Relations* (1837), ed. Robert F. Berkhofer, Jr., 2 vols. in 1 (New York, 1968), 336–76 *passim*; and J. D. B. DeBow, *The Interest in Slavery of the Southern Non-Slaveholder* (Charleston, S.C., 1860), 4–8. The economic argument was potentially the mildest and most equivocal defense of bondage, since such a rationale was entirely compatible with the idea that unfree labor was wrong in theory but necessary under particular circumstances.

<sup>33</sup> M. M. Shcherbatov, “Zamechaniia Shcherbatova na bol’shoi nakaz Ekateriny,” in his *Neizdannnye sochineniia*, 55–56, and “Primechanie na 13-iu stat’iu,” 196–97; “Mnenie ob osvobozhdenii krest’ian” (1767), in *Russkii arkhiv* (1871), 288–91; Komissarenko, “Proekt vvedeniia lichnoi krepostnoi zavisimosti ekonomicheskikh krest’ian”; Karamzin, “Pis’mo sel’skago zhitelia,” 569–74; F. V. Rostopchin, “Zamechanie Grafa F. V. Rostopchina na knigu g-na Stroinovskago” (1811), *Chteniia v Imperatorskom obshchestve istorii i drevnostei rossiiskikh*, 33, no. 2, pt. 5 (1860): 205, 211–15; Frederika Teute Schmidt and Barbara Ripel Wilhelm, “Early Proslavery Petitions in Virginia,” *William and Mary Quarterly*, 3d ser., 30 (1973): 138–40; Dew, “Professor Dew on Slavery,” 437–40; Harper, “Slavery in the Light of Social Ethics,” 617–23; [Holland] *A Refutation of the Calumnies against the Southern and Western States*, 61–85; and Estes, *A Defence of Negro Slavery*, 232–52.

<sup>34</sup> Boltin, *Primechaniia*, 235–36. Also see Shcherbatov, “Zamechaniia Shcherbatova na bol’shoi nakaz Ekateriny,” 55; and Shishkov, *Zapiski, mneniia, i perepiski*, 120–24.

gave the South a bad name. "Theoretically, slavery has been abolished in most countries," he admitted; "but, practically, it exists almost every where—but without the responsibilities, interests, humanities, and sympathies of [Southern] slavery."<sup>35</sup>

DESPITE MANY OBVIOUS INCONSISTENCIES, there is a common thread running through virtually all of the defenses of servitude in Russia and the United States: the assumption that men are naturally unequal. This assumption usually appears only implicitly in the arguments on behalf of forced labor, but at times it is quite explicit. Prince Shcherbatov, for example, boldly refuted the concept of natural equality. "Not one person is completely like another," he wrote; "and, where this similarity is lacking, so too is equality." Half a century later, Admiral Mordvinov made the same point. "Only disorganized, wild society offers equality of rights, conditions, and powers," he explained. "Such is the condition of all Asiatic peoples."<sup>36</sup> Nineteenth-century Southerners were sometimes even more explicit in combating head-on the basic precepts of Jeffersonianism. "Man is born to subjection," wrote South Carolina's Chancellor Harper; "it is the very basis of his nature, that the strong should control the weak and ignorant." Hammond was equally direct and forceful in refuting the doctrine of natural, unalienable rights: "I repudiate, as ridiculously absurd, that much lauded but nowhere accredited dogma of Mr. Jefferson, that 'all men are born equal,' " he declared. "It is a wretched and insecure government which is administered by its most ignorant citizens, and those who have the least stake under it."<sup>37</sup> Regardless of the variety of ways used to defend inequality (race, class, lineage, wealth, ability, intellect, or moral capacity), the essential premise of all who defended bondage was that all men were *not* created equal.

Because the proponents of unfree labor were forced to defend inequality in an era of natural rights, they were compelled to challenge many of the dominant intellectual currents of their time, to reject the French Enlightenment, Jeffersonianism, and progress. The defense of slavery and serfdom therefore led inevitably to reactionary views on most other social questions. Although some proslavery advocates in the United States South were able to stay in tune with the times by the simple expedient of excluding blacks from the realm of humanity,<sup>38</sup> the logic of defending slavery left most polemicists uncomfortable with talk of any kind of reform.

<sup>35</sup> Hughes, *Treatise on Sociology*; Hammond, "Speech on the Admission of Kansas," 319; and Estes, *A Defence of Negro Slavery*, 130. Also see Cobb, *An Inquiry into the Law of Negro Slavery*, cxii–cxxxii, cxix–ccxii; and the editor's Introduction to "Northern and Southern Slavery," *Southern Literary Messenger*, 7 (1841): 341. On Southern discomfort with the term "slavery," see Kenneth S. Greenberg's perceptive "Revolutionary Ideology and the Proslavery Argument: The Abolition of Slavery in Antebellum South Carolina," *JSH*, 42 (1976): 365–84.

<sup>36</sup> Shcherbatov, "Razmyshlenie o dvorianstve," 222; and Mordvinov, "Mnenie Admirala Mordvinova po rabstvu krest'ian, v 1833 godu," *Chteniia v Imperatorskom obshchestve istorii i drevnostei rossiskikh*, 30, no. 3, pt. 5 (1859): 56.

<sup>37</sup> Harper, "Harper on Slavery," 8; and Hammond, "Slavery in the Light of Political Science," 637–38. Also see W. G. Bean, "Anti-Jeffersonianism in the Antebellum South," *North Carolina Historical Review*, 12 (1935): 103–24.

<sup>38</sup> See Fredrickson, *The Black Image in the White Mind*, 61–94.



In 1857, British traveler Barbara Bodichon summed up in her diary a conversation she had with several Southerners while on a Mississippi River steamboat. "There is evidently a feeling," she wrote, "that Abolition and Woman's Rights are supported by the same people and the same arguments, and that both are allied to atheism—and all these slave owners are very religious people."<sup>39</sup> Her observation was perceptive. Almost every major antebellum defender of slavery insisted on tying abolitionism to a host of other "isms"—"heresies" that threatened social peace and stability. George Frederick Holmes was typical in his suggestion that in the North, "where Fourierism, and Proudhonism, Free Love, and Total Abstinence, and all the other modern forms of philanthropic innovation have found numerous and enthusiastic votaries, an exaggerated and distorted idea of the nature and functions of liberty has inspired the multitudinous heresy of Abolitionism."<sup>40</sup> Rejecting the optimistic spirit of Northern reform, Hammond deplored the democratic course of the nineteenth century, with the ascendancy of "the MOB—THE SANS-CULOTTES. Preaching as their watchword that now prostituted sentiment 'that all men are born free and equal,' they have rallied to their standard the ignorant, uneducated, semi-barbarous mass which swarms and starves upon the face of Europe!" Only one bulwark remained against this leveling surge: the slave South. Scoffing at abolitionist charges of a slave-owning aristocracy, Hammond replied, "I accept the terms. *It is a government of the best*, combining all the advantages of the old world."<sup>41</sup>

Russian defenders of serfdom denounced the democratic spirit in much the same terms. Like their American counterparts, they saw themselves defending not simply an institution but a conservative regime, one threatened by equality, democracy, and revolution. As early as the 1780s, Count S. R. Vorontsov identified the main enemy as "this spirit of reform and universal equality preached for fifty years by the economists and encyclopedists in France." Half a century later Admiral A. S. Shishkov vigorously protested against the view that the "spirit of the times" demanded reform of serfdom. "By the *spirit of the times*," he asserted, "is often meant a general willfulness and disobedience." Contrasting the social harmony of Russia with the turmoil of Western Europe, he asked rhetorically, "Why changes in laws, changes in customs, changes in manner of thought? And whence these changes? From the schools and philosophizing of those countries where these disorders, these insurrections, this insolence of thought . . . reign supreme." The only cure, he suggested, was strict censorship to guard against the spreading disease of free thought. In a fervent attack on the concept of a natural right to freedom, Count F. V. Rostopchin explained that, although the term "freedom" was appealing, "it is not the natural condition of a person," for all members of society were dependent on one another. "The first consequence of

<sup>39</sup> Barbara Leigh Smith Bodichon, *An American Diary, 1857–8*, ed. Joseph W. Reed, Jr. (London, 1972), 61.

<sup>40</sup> George Frederick Holmes, "Theory of Political Individualism," *DeBow's Review*, 22 (1857): 134. Also see Harper, "Slavery in the Light of Social Ethics," 580–85; Hammond, "Slavery in the Light of Political Science," 117, 149–50; Stringfellow, "Statistical View of Slavery," 524–28, 540–42; Simms, "The Morals of Slavery," 264; Hundley, *Social Relations in Our Southern States*, 16–17; and Fitzhugh, *Cannibals All*, 6, 9–11, 85–106, 190–98, 213–16.

<sup>41</sup> Hammond, "Speech on the Justice of Receiving Petitions," 43–45.

freedom is willfulness," he noted, "the second is disobedience, and the third is revolt against all authority." In the first half of the nineteenth century Russians and Southerners liked to contrast the harmony of class relations in their societies with the chaos of the free-labor market in Western Europe and the North.<sup>42</sup> For both, the defense of servitude was an integral part of the general defense of a world threatened by change.

WHILE ESSENTIALLY SIMILAR IN CHARACTER, the arguments for slavery and serfdom differed in their development over time. This difference became apparent, however, only after the beginning of the nineteenth century. Before 1800 American and Russian thought on unfree labor followed remarkably similar paths. In both countries there was little discussion of bondage before the 1760s;<sup>43</sup> challenges to slavery and serfdom were few, but so too were articulated defenses of institutions taken largely for granted. In both countries, complacency and silence gave way to concern and debate during the last third of the eighteenth century. Although those who raised the issue of forced labor rarely called for immediate abolition, they did broach the possibility of gradual emancipation in the future and discuss ways of more immediately limiting cruel and arbitrary treatment of the bondsmen.<sup>44</sup> They also aroused the angry opposition of those who insisted no change was needed.

This parallel course came to an abrupt end in the nineteenth century. Cautious antislavery sentiment, common among spokesmen of the Upper South, gradually evaporated and was replaced, at first, by an awkward, hesitant, even reluctant defense of slavery. As late as 1826 Presbyterian missionary Timothy Flint was surprised to find that planters defended existing conditions primarily

<sup>42</sup> Vorontsov, "Zapiska Grafa S. R. Vorontsova o dvorianstve," *Arkhiv kniazia Vorontsova*, 16: 299–300; Shishkov, *Zapiski, mneniia, i perepiski*, 121, 129; Shishkov to the Emperor, December 12, 1836, *Chteniiia v Imperatorskom obshchestve istorii i drevnostei rossiiskikh*, 71, no. 3, pt. 2 (1868): 121–28; and Rostopchin, "Zamechanie Grafa F. V. Rostopchina na knigu g-na Stroinovskago," 204–05. Also see Count D. P. Buturlin to S. R. Vorontsov, February 22, April 4, and July 30, 1803, in *Arkhiv kniazia Vorontsova*, 22: 334–36, 343–45, 364–66. Although the defense of serfdom was extremely useful to supporters of the status quo, it was not absolutely essential. Under Nicholas I, government spokesmen sometimes contrasted conservative Russia and the revolutionary West—using the official line of "Autocracy, Orthodoxy, and Nationality"—without reference to serfdom; and most Slavophiles, although contemptuous of Western decadence, were at least in theory critical of serfdom. See Edward C. Thaden, *Conservative Nationalism in Nineteenth-Century Russia* (Seattle, 1964), esp. 19–20; and Nicholas V. Riasanovsky, *Nicholas I and Official Nationality in Russia, 1825–1855* (Berkeley and Los Angeles, 1961), esp. 12, 134–43, 167–68, and *Russia and the West in the Teachings of the Slavophiles* (Cambridge, Mass., 1952), 91–119, 136–40.

<sup>43</sup> Jenkins, *Pro-Slavery Thought*, 3; David Brion Davis, *The Problem of Slavery in Western Culture* (Ithaca, N.Y., 1966), 125–50; and Semevskii, *Krest'ianskii vopros*, 1: 1–12.

<sup>44</sup> On the debate over slavery during the Revolutionary era, see David Brion Davis, *The Problem of Slavery in the Age of Revolution, 1770–1823* (Ithaca, N.Y., 1975), 82–326, and *The Problem of Slavery in Western Culture*, 365–445; Jordan, *White over Black*, 287–304, 429–81; William W. Freehling, "The Founding Fathers and Slavery," *AHR*, 77 (1972): 81–93; Robert McColley, *Slavery and Jeffersonian Virginia* (Urbana, Ill., 1964), 114–40; Duncan J. MacLeod, *Slavery, Race, and the American Revolution* (Cambridge, 1975), 14–47; John Chester Miller, *The Wolf by the Ears: Thomas Jefferson and Slavery* (New York, 1977); and Tise, "Proslavery Ideology," 80–110. On the peasant question in Catherine II's Russia, see Semevskii, *Krest'ianskii vopros*, 1: 14–213; Beliauskii, *Krest'ianskii vopros v Rossii nakanune vosstaniia E. I. Pugacheva*; Jones, *The Emancipation of the Russian Nobility*, 135–42; Augustine, "Economic Attitudes and Opinions"; Dukes, *Catherine the Great and the Russian Nobility*, 91–95; Mavrodin, *Klassovaia bor'ba*, 166–83; and A. I. Pashkov, ed., *A History of Russian Economic Thought: Ninth through Eighteenth Centuries* (1955), trans. John M. Letiche et al. (Berkeley and Los Angeles, 1964), 475–575.

on the grounds of necessity, precedent, and the dangers of sudden emancipation, not the virtues of slavery. "I have never yet heard one," he exaggerated, "who does not admit that slavery is an evil and an injustice and who does not at least affect to deplore the evil."<sup>45</sup> Then, during the thirty years preceding the Civil War, the South produced an extraordinary torrent of proslavery propaganda—propaganda that was for the most part bold, unapologetic, and insistent on the positive virtues of slavery. Whereas previous proslavery advocates had stressed the "practical" arguments of expedience, economy, and race, the new polemicists, who included many of the South's best minds, increasingly based their case on the higher ground of social theory: slavery was simply the best way to organize society. By the 1850s, Southern whites seemed so committed to their peculiar institution that a lone dissenter such as Hinton Helper found it expedient to leave the South altogether.<sup>46</sup>

In Russia, however, the trend that had begun in the eighteenth century continued into the nineteenth. Although some noblemen still spoke out in defense of serfdom and unarticulated sentiment in favor of maintaining the status quo remained strong, especially in the provinces, the balance gradually tipped in favor of those dissatisfied with the institution. By the 1840s, free-labor ideas had spread widely among the educated nobility of Moscow and St. Petersburg, and public defense of serfdom became increasingly rare. (Significantly, virtually all of the defenses of serfdom cited above date from before 1840.) Thus, Russia never experienced the kind of militant proslavery movement that reigned in the antebellum South. If the entire American South seemed to rally around slavery, educated Russians—including many high level government officials—became increasingly convinced that serfdom was a backward system that must, somehow, be abolished.<sup>47</sup> In 1839 the annual report of the Third Department, or political police, suggested that the time had come for the government to begin preparing for eventual emancipation rather "than to wait until it begins from

<sup>45</sup> Timothy Flint, *Recollections of the Last Ten Years* (1826), ed. C. Hartley Grattan (New York, 1932), 329. For similar observations, see Morris Birkbeck, *Notes on a Journey in America, from the Coast of Virginia to the Territory of Illinois* (1817; reprint ed., Ann Arbor, Mich., 1968), 16; Harriet Martineau, *Society in America* (1837), ed. Seymour Martin Lipset (abridged ed., New York, 1962), 189; and Frances Anne Kemble, *Journal of a Residence on a Georgian Plantation in 1838–1839* (Chicago, 1969), 130.

<sup>46</sup> See Jenkins, *Pro-Slavery Thought*, 48–300; Genovese, *The World the Slaveholders Made*, pt. 2; Fredrickson, *The Black Image in the White Mind*, 43–96; Smith, *In His Image*, 129–64; and Takaki, *A Pro-Slavery Crusade*, *passim*. On the South as a closed society, see Clement Eaton, *The Freedom-of-Thought Struggle in the Old South* (New York, 1964).

<sup>47</sup> Among the great Russian novelists of the nineteenth century, Nicholas Gogol was virtually alone in his defense of serfdom, and some, such as Ivan Turgenev, eluded heavy government censorship and openly ridiculed the institution. For Gogol's defense of serfdom as a God-given institution designed for the well-being of all, see his "Russkii pomeshchik," originally published in his *Vybrannye mesta iz perepiski s druž'iami* (1847), reprinted in N. V. Gogol', *Sobranie sochinenii v semi tomakh*, 6 (Moscow, 1967): 316–23. For Turgenev's most damning portrait of serfdom, see his *A Sportsman's Notebook*, trans. Charles Hepburn and Natasha Hepburn (London, 1950), and, for Turgenev as an opponent of serfdom and autocracy, see Harry Hershkovitz, *Democratic Ideas in Turgenev's Works* (New York, 1932). In contrast, leading antebellum Southern writers—one cannot call them great—commonly extolled the peculiar institution. For treatment of a prominent example, see Jon L. Wakelyn, *The Politics of a Literary Man: William Gilmore Simms* (Westport, Conn., 1973). Because most Southern writers advocated slavery, Hannah Stern Goldman, who has stressed the parallels between antibondage themes in American and Russian nineteenth-century fiction, was forced to compare Russian novels with those written in the North; see her "American Slavery and Russian Serfdom: A Study in Fictional Parallels" (Ph.D. dissertation, Columbia University, 1955).

below, from the people.” The report called for quiet deliberation, “without noise and without loud words,” but concluded that “everyone is agreed” on the need for reform.<sup>48</sup> When in 1857 Alexander II made public his decision to go ahead with emancipation, noblemen grumbled and dragged their feet, but there was no public opposition, no threat that serfowners might refuse to accept the reforms.<sup>49</sup>

THAT A BASIC CONTRAST EXISTS here is clear; the obvious question is how to account for it. Although that question cannot be definitively answered in a few pages, several factors can be discerned that contribute to an explanation. They also reveal much not only about American slavery and Russian serfdom but about how social elites behave when their privileges are under attack as well.

The most obvious is that of race. Not only did proslavery spokesmen often couch their arguments in racial terms but Southern whites were also, in general, so imbued with the consciousness of race that they found it impossible to contemplate emancipation of blacks in a white society. Travelers to the antebellum South found that many whites who were distinctly unenthusiastic about slavery balked at the notion of turning blacks loose in the South. As one poor white told Frederick Law Olmsted, “I reckon the majority would be right glad if we could get rid of the niggers. But it wouldn’t never do to free ’em and leave ’em here. . . . Nobody couldn’t live here then.”<sup>50</sup> That slavery in the United States was racial instead of merely social clearly served to inhibit the growth of moderate opposition and to make Southern whites more receptive to the proslavery appeal.

If Russian defenders of serfdom invented essentially racial arguments to serve their cause and regarded peasants as a different people, a qualitative gap still separated both the perceptions of the differences between masters and bondsmen in the two countries and the perceptions of the threat inherent in emancipation. Blacks were not merely different: they were outsiders in a country where everyone else was an insider or a potential insider. Hence, free blacks seemed like such an anomaly. Blacks were aliens—Africans deposited against their will in a foreign land—and, despite protestations to the contrary, most whites always regarded them as such. How else could one speak of sending “back” to Africa people who in most cases were third- and fourth-generation Americans? Although a Southern planter might refer to his slaves as “my people,” the term

<sup>48</sup> “Iz otcheta III Otdeleniia o vnutrennem sostoianii i o krest’ianskom dvizhenii v strane v sviazi s pozharimi,” in A. V. Predtechenskii, ed., *Krest’ianskoe dvizhenie v Rossii v 1826–1849 gg.: Sbornik dokumentov* (Moscow, 1961), 344–45. “I have never been either an ultra-liberal or a carbonari . . . but I have always detested personal slavery, and I still detest it, and deplore its continuation among us and everywhere I see it,” wrote Prince M. S. Vorontsov, whose father had defended serfdom; Vorontsov to P. D. Kiselev, Tiflis, October 20, 1847, in *Arkhiv kniazia Vorontsova*, 38: 149–50.

<sup>49</sup> Semevskii, *Krest’ianskii vopros*, 1: 236–end, 2: *passim*; Terence Emmons, *The Russian Landed Gentry and the Peasant Emancipation of 1861* (Cambridge, 1968), 29–35; Daniel Field, *The End of Serfdom: Nobility and Bureaucracy in Russia, 1855–1861* (Cambridge, Mass., 1976), esp. 102–40; and Nicholas V. Riasanovsky, *A Parting of the Ways: Government and the Educated Public in Russia, 1801–1855* (Oxford, 1976), 262–63.

<sup>50</sup> Olmsted, *A Journey in the Back Country* (1860; reprint ed., New York, 1970), 203. Olmsted noted that “these views of slavery seem to be universal among people of this class. They were represented to me at least a dozen times.” Also see Fredrickson, *The Black Image in the White Mind*, 56–90 *passim*.

"the people" was always reserved for whites who formed the body politic. In a world where all free men were politically equal—and white—the prospect of freeing black slaves alarmed slaveholders and nonslaveholders alike.<sup>51</sup>

Russian peasants, however, were not outcasts but merely the lowest level of a stratified society. White Americans could think of the United States as an all-white country, but Russia without peasants was inconceivable. Not only did they constitute over four-fifths of the population, they were the essence of Russia. When Russians spoke of "the people," they meant precisely the peasants. For the peasants—whether bound or free—to constitute "the people" did not threaten the Russian established order in the way that citizenship for blacks threatened the white South, because Russia was a hierarchical society, composed of legally established castes or estates, without the slightest pretension to democracy or equal rights. Emancipating blacks in the United States entailed the radical step of extending citizenship. Emancipating serfs, however, constituted a less extreme measure, for an emancipated serf would still be a peasant, and no one imagined that emancipation would make a peasant the equal of a noble. In short, the combination of race and democracy served to reinforce the commitment to slavery.<sup>52</sup>

There was another way in which the lack of democracy served to undercut the Russian nobility's commitment to serfdom. Because Russia had a bureaucratic government, organized on essentially military lines of command, the gentry was not in a position either to shape or to resist government policy. During the decades preceding emancipation the top ranks of government—the tsars and many of their close advisors—were committed to a policy of gradual reform. Southern whites would have elected new leaders, but Russian *pomeshchiki* had no such choice; they simply sulked or ignored the matter while successive government committees considered how to handle the "peasant question." Even when faced with emancipation in the late 1850s, reluctant noblemen did not think of refusing; instead, they contented themselves with minor obstructions and delays while insisting that the final settlement be as favorable as possible to their economic interests.<sup>53</sup>

Such was not, of course, the response of Southern slaveholders when slavery was assaulted. Surely one important clue to Southern response lies in the nature

<sup>51</sup> Several authors have, in varying fashions, stressed the connection among race, democracy, and slavery in the antebellum South. See Carl N. Degler, *Neither Black nor White: Slavery and Race Relations in Brazil and the United States* (New York, 1971), 256–60, and "The Irony of American Negro Slavery," in Harry P. Owens, ed., *Perspectives and Irony in American Slavery* (Jackson, Miss., 1976), 3–12; Edmund S. Morgan, *American Slavery, American Freedom: The Ordeal of Colonial Virginia* (New York, 1975), 369–86; and MacLeod, *Slavery, Race, and the American Revolution*, 148–84. On Southern democracy, see William J. Cooper, Jr., *The South and the Politics of Slavery, 1828–1856* (Baton Rouge, 1978), esp. 23–42.

<sup>52</sup> For a similar interpretation, see Degler, *Neither Black nor White*, 256–60.

<sup>53</sup> Semevskii, *Krest'ianskii vopros*, 1: 236–end, 2: *passim*; Field, *The End of Serfdom*, 102–40, 172–232, 265–323, 359–60; P. A. Zaionchkovskii, *Otmena krepostnogo prava v Rossii* (3d ed., Moscow, 1968), 1–123; and W. Bruce Lincoln, *Nicholas I: Emperor and Autocrat of All the Russias* (London, 1978), 187–95. Noblemen's attitudes during the preparations for emancipation can be traced in the pages of *Zhurnal zemlevaladel'tsev*, which appeared twice monthly in 1858 and 1859. On the prereform bureaucracy, see S. Frederick Starr, *Decentralization and Self-Government in Russia, 1830–1870* (Princeton, 1972), 9–50; and P. A. Zaionchkovskii, "Gubernskaia administratsiia nakanune Krymskoi voyny," *Voprosy istorii* (1975), no. 9: 33–51.



of the challenge: it was open, and it came from without. Opponents of slavery were able publicly—through meetings, petition drives, pamphlets, newspaper articles and editorials, and political speeches—to challenge both the legitimacy of slaveowning and the morality of slaveowners. The outspoken defense of slavery thus arose largely in reaction to these attacks upon it. Equally important was the sectional nature of the opposition. Because antebellum slavery was Southern and abolitionism Northern, the attack on slavery appeared to be an attack on the South itself. The defense of slavery would undoubtedly have been quite different but for the sectional character of the assault on it.<sup>54</sup>

Russia, however, was not divided into free and unfree sections. Although serfs were more numerous in some areas than in others, serfdom was a national institution. Equally significant, Russians, because of the nature of their government, never conducted a public debate over the peasant question. Proposals for abolishing or modifying serfdom, although widely discussed in the highest circles, were always confined to secret committees whose proceedings were not made public. Heavy censorship meant that only the most Aesopian remarks critical of Russian institutions could appear in print, and nothing like the American abolitionists' denunciation of slaveholders as sinners was possible.<sup>55</sup> As a result, serfowners never had—or got—to defend themselves as American slaveowners did. Even had they wanted to, they would not have been able to play the same ideological role as American proslavery spokesmen: there was no Russian "public" to which to appeal, and censorship of arguments defending serfdom was almost as rigid as censorship of polemics against it. The absence of a free press and of any tradition of democratic debate on policy thus precluded a full development of proserfdom thought.<sup>56</sup>

Thus, the flowering of the antebellum defense of bondage in the U.S. South, in contrast to its withering in Russia, is at least partially explicable in terms of four concrete differences between the two societies, four characteristics that were present in the United States and absent in Russia: (1) a racial distinction between owner and owned; (2) a democratic political system; (3) freedom of the press; and (4) the sectional nature of servitude. These four are functionally related to a fifth, which subsumes the other four under it and is the most important of all. This crucial difference involves the independence of the master class and the strength of its civilization.

Southern planters were stronger, were more independent, than their Russian counterparts in relation both to their government and to their property. Because slaveowners lived under a democratic political system whose government repre-

<sup>54</sup> Jenkins, *Pro-Slavery Thought*, 65–66, 104; Fredrickson, *The Black Image in the White Mind*, 3; Morrow, "The Proslavery Argument Revisited," *passim*; Barrington Moore, Jr., *Social Origins of Dictatorship and Democracy: Lord and Peasant in the Making of the Modern World* (Boston, 1966), 121–22; Jesse T. Carpenter, *The South as a Conscious Minority, 1789–1861* (New York, 1930), 21–76, 173–220; and Cooper, *The South and the Politics of Slavery*: 58–65.

<sup>55</sup> Former Decembrist Nicholas Turgenev emphasized precisely this contrast between Russian silence and an outspoken American abolitionism; see his *La Russie et les russes*, 3 vols. (Brussels, 1847), 2: 113.

<sup>56</sup> Semevskii, *Krest'ianskii vopros*, 1: 28–33, 393–410, 419–28, 2: 325–40, 349–61; and Field, *The End of Serfdom*, 39–40.

sented their interests, because blacks were outsiders in a white society, and because slavery in the antebellum period was a sectional institution, Southern slaveowners constituted a local ruling class with a powerful sectional culture. As Eugene D. Genovese has cogently argued, they created a confident civilization based on the conservative virtues of paternalism, *noblesse oblige*, order, and social cohesion that contrasted sharply with the slave societies of the Caribbean.<sup>57</sup> It also contrasted sharply with that of imperial Russia.

If Southern slaveowners were able to develop their own independent sectional culture, one might argue that the central problem of Russian *pomeshchiki* was their dependence—on both their serfs and their government. Of course, Russian noblemen, both as individuals and as a class, were powerful men, and it may seem paradoxical to speak of them as a dependent group. But they lacked almost all of the elements of independence enjoyed by American planters. Unlike slaveowners, most of whom lived on their farms or plantations (or in nearby towns) and took an active role in governing both their slaves and their communities, Russian noblemen maintained a largely absentee mentality. This was dictated in part simply by prevailing demographic conditions, which made it much more difficult for prominent Russian *pomeshchiki* to develop the kind of local ties that were common among American planters. Whereas the vast majority of Southern slaves were held in units of under one hundred slaves, almost half of all serfs lived on holdings of over one thousand peasants, and four-fifths of them lived on holdings of over two hundred.<sup>58</sup> Wealthy, influential noblemen usually owned numerous estates, scattered among several provinces, each of which they visited only occasionally, if at all.

Even holders of smaller numbers of serfs frequently lived in Moscow, St. Petersburg, or a provincial capital, leaving the management of their estates to stewards. Although noblemen won their freedom from compulsory state service during the second half of the eighteenth century, they continued to look upon themselves as a serving class, and most noblemen continued to regard government service as the norm, retiring to one of their estates only as old men. As a result, they never developed the kind of corporate spirit or local commitments that were common among Western European aristocrats—or Southern planters. Of course, a large number of Russian landlords were not absentee owners; many of the less exalted noblemen owned only a few serfs, and at the bottom of the noble hierarchy was a significant group of petty landowners who were reduced to working in the fields alongside their peasants.<sup>59</sup> But emphasizing their phys-

<sup>57</sup> Genovese, *The World the Slaveholders Made*, esp. chap. 2, *The Political Economy of Slavery: Studies in the Economy & Society of the Slave South* (New York, 1965), esp. 28–31, Roll, Jordan, Roll: *The World the Slaves Made* (New York, 1974), 1–97, and “Slavery—The World’s Burden,” in Owens, *Perspectives and Irony in American Slavery*, 27–40. Also see Moore, *Social Origins of Dictatorship and Democracy*, 122; William K. Scarborough, “Slavery—The White Man’s Burden,” in Owens, *Perspectives and Irony in American Slavery*, 103–35; Rollin G. Osterweis, *Romanticism and Nationalism in the Old South* (1949; reprint ed., Baton Rouge, 1967); Robert E. Shalhope, “Race, Class, Slavery, and the Antebellum Southern Mind,” *JSH*, 37 (1971): 557–74; and Clement Eaton, *The Growth of Southern Civilization, 1790–1860* (New York, 1961), esp. 1–48, 98–124, 295–324.

<sup>58</sup> A. Troinitskii, *Krepostnoe naselenie v Rossii, po 10-i narodnoi perepisi* (St. Petersburg, 1861), 45, 65.

<sup>59</sup> See Blum, *Lord and Peasant in Russia*, 349, 375–76; Emmons, *The Russian Landed Gentry*, 4–5; and Richard Pipes, *Russia under the Old Regime* (New York, 1974), 175–78. In 1858, 77.5 percent of all *pomeshchiki* in Euro-

ical presence risks obscuring their psychological absence. Not only were the richest and most powerful lords, whose lifestyle and conduct influenced the social aspirations of all noblemen, usually far removed from their estates; but, more importantly, even most resident *pomeshchiki* possessed an absentee mentality. Their hearts were still in Moscow or St. Petersburg, not in the countryside; the latter belonged to the peasants. As Daniel Field has aptly written, the Russian village “was a peasant world; the pomeshchik was almost an outsider even on his ancestral estate.”<sup>60</sup>

The consequences of this contrast—between a resident planter class and a largely absentee class of servitors—are numerous; one of the most important concerns the nature of relations between masters and bondsmen. But the consequence of interest here involves the defense of bondage. Unlike American planters, Russian noblemen were simply not in a position to defend forced labor; they lacked the independence to do so. They also lacked much of the incentive, for many *pomeshchiki* were essentially *rentiers*, deriving an income from, but otherwise unconcerned about, their estates.<sup>61</sup> Whereas emancipation threatened the entire world of Southern slaveowners, it primarily threatened the Russian noblemen’s livelihoods. If a way could be found to safeguard their immediate economic interests, the noblemen had relatively little to lose from abolition. Therefore, during the preparations for emancipation in the late 1850s, they concentrated not on opposing the new order but on securing the best possible terms for themselves under it. And the terms they ultimately secured were generous indeed.<sup>62</sup>

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pean Russia owned two hundred or fewer serfs (one hundred or fewer “male souls”) and were therefore unable, according to the exaggerated standards of the time, to live in proper gentry style; 43.6 percent owned forty or fewer serfs (the poorest of these had lifestyles that differed little from those of prosperous peasants); and 3.4 percent of all noblemen owned no serfs at all; Troinitskii, *Krepostnoe naselenie*, 45.

<sup>60</sup> Field, *The End of Serfdom*, 22. On the serving mentality and lack of independence of the Russian nobility, see Romanovich-Slavatinskii, *Dvorianstvo v Rossii*, esp. 115–208, 402–10, 495–500; S. A. Korf, *Dvorianstvo i ego soslovnnoe upravlenie za stoletie 1762–1855 godov* (St. Petersburg, 1906), esp. 212–14, 287–363, 391–94, 449–59, 635–51; Max Beloff, “Russia,” in A. Goodwin, ed., *The European Nobility in the Eighteenth Century: Studies of the Nobilities of the Major European States in the Pre-Reform Era* (London, 1953), 172–89; Pipes, *Russia under the Old Regime*, 98, 172–79; and Robert David Givens, “Servitors or Seigneurs: The Nobility and the Eighteenth-Century Russian State” (Ph.D. dissertation, University of California, Berkeley, 1975).

<sup>61</sup> The contrast between American planters and Russian *pomeshchiki* is clearly revealed in the instructions and rules they composed for running their estates. In contrast to the American regulations, which usually provided detailed prescriptions for the care and handling of slaves, the Russian instructions focused on agricultural matters, and on the extraction of income from the peasants. Compare, for example, *pomeshchik* Alekseev Gvozdev’s “Prikaznye punkty . . .” (composed ca. 1811–21), in A. Cherepnin, ed., “Prikazy starostam,” *Trudy vysschaishie uchrezhdennoi Riazanskoi Uchenoi Arkhiinovoi Komissii za 1904 god*, 19: 82–93, with the “Rules on the Rice Estate of P. C. Weston, S.C.” (1856), in Ulrich B. Phillips, ed., *Plantation and Frontier*, vols. 1–2 of John R. Commons et al., eds., *A Documentary History of American Industrial Society* (Cleveland, 1910), 1: 115–22. For a contemporary view of the absence of *pomeshchik* paternalism, see Turgenev, *La Russie et les russes*, 3: 68. Michael Confino, however, has taken an opposing position in his important study; see his *Domaines et seigneurs en Russie vers la fin du XVIII<sup>e</sup> siècle: Étude de structures agraires et de mentalités économiques* (Paris, 1963), esp. 103–04.

<sup>62</sup> See the works cited in notes 49, 53, above. Also see Zaionchkovskii, *Otmena krepostnogo prava*, 125–50; and B. G. Litvak, *Russkaia derevnia v reforme 1861 goda: Chernozemnyi tsentr, 1861–1895 gg.* (Moscow, 1972). Emancipation in Central European countries, where seigneurs generally continued to maintain their class prerogatives, served as a very different kind of example to the Russian lords from that provided for American planters by the West Indian emancipation. On the Russian government’s interest in Central European agrarian reform measures, see Blum, *The End of the Old Order*, 368.

USEFUL CONCLUSIONS CAN BE DRAWN from both the similarity of the Russian and American defense of servitude and the difference in the development of that defense. The logic of defending unfree labor produced similar arguments—and similar assumptions about the nature of the good society—despite major cultural and economic differences between Russia and the United States and despite unique features of American slavery and Russian serfdom that might have influenced the nature of the defense. In short, slave-owning classes shared certain important assumptions that cut across variations in region, culture, and economy.<sup>63</sup>

Nevertheless, these variations operated to accentuate the defense of bondage in the United States and to undercut it in Russia. Racial, political, cultural, and demographic differences acted to produce two contrasting master classes: one that was independent, autonomous, and socially dominant, and another that was dependent, timid, and socially marginal. While the antebellum South was a slaveholders' world, prereform Russia was a peasant world. As a consequence, the former was able to sustain a proslavery ideology that was unique in volume, subtlety, and sophistication, while the latter developed arguments that were never as finely wrought and that gradually lost their force and persuasiveness. The Russian defenses of serfdom were more obviously self-serving and lacked the boldness, imagination, sweep, and abstraction of their American counterparts. Quite simply, Russians did not make as compelling a case for their form of bondage.<sup>64</sup>

If analysis of the contrasting courses of master class ideologies helps us understand better the impact of specific social and cultural attributes on systems of forced labor, it also raises a more general question: how will beneficiaries of a particular social system—in this instance, slaveowners—behave when that system is threatened? Although a dominant class can be expected to defend its prerogatives, this defense is likely to be more stubborn and persistent in some circumstances than in others. I would suggest that, in general, commitment to a social system will be greatest when an entire way of life rather than simply an investment is at stake. (Note, for example, how little resistance was offered by planters to compensated emancipation in the British West Indies, where plantations were pre-eminently speculations for absentee investors.<sup>65</sup>) Ultimately,

<sup>63</sup> See Genovese, *The World the Slaveholders Made*, pt. 1, esp. 3–7, 112; and Davis, *The Problem of Slavery in Western Culture*, 29–121, 223–61.

<sup>64</sup> There is reason to believe that the United States—not Russia—was unusual here. For the suggestion that the antebellum South, as the most paternalistic of New World slave societies, developed the most elaborate and well-rounded defense of slavery, see Genovese, *The World the Slaveholders Made*, pt. 1, esp. 100–03. For the failure of Jamaican planters to develop the argument that slavery was a positive good, see Philip D. Curtin, *Two Jamaicas: The Role of Ideas in a Tropical Colony, 1830–1860* (Cambridge, Mass., 1955), 62–69.

<sup>65</sup> See Lowell Joseph Ragatz, *The Fall of the Planter Class in the British Caribbean, 1763–1833: A Study in Social and Economic History* (London, 1928), 3–80, 453–54; Curtin, *Two Jamaicas*, 15–18, 55, 62–69, 92–95; and George M. Fredrickson, “After Emancipation: A Comparative Study of White Responses to the New Order of Race Relations in the American South, Jamaica, & the Cape Colony of South Africa,” in David G. Sansing, ed., *What Was Freedom's Price?* (Jackson, Miss., 1978), 76–80, 86. C. Vann Woodward has likewise suggested that “the end of slavery in the South can be described as the death of a society, though elsewhere it could more reasonably be characterized as the liquidation of an investment”; Woodward, “The Price of Freedom,” in *ibid.*, 97.

Southern slaveowners rose to the defense of their peculiar institution—with both words and arms—because they had more than an economic interest in the outcome. Russian *pomeshchiki* failed to do so because they lacked the means and because, as they recognized, abolition raised a potential threat to their pocketbooks that, given the balance of forces, they could successfully overcome. From their own points of view, both planters and *pomeshchiki* made the “correct” decision.



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*AHR Forum*  
Marc Bloch and Comparative History

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ALETTE OLIN HILL  
and  
BOYD H. HILL, JR.

Finally, of what use is linguistics? Very few people have clear ideas on this point, and this is not the place to specify them. But it is evident, for instance, that linguistic questions interest all who work with texts—historians, philologists, etc. Still more obvious is the importance of linguistics to general culture: In the lives of individuals and societies, speech is more important than anything else.<sup>1</sup>

FEW HISTORIANS OF THE TWENTIETH CENTURY have had as much impact upon their profession as has Marc Bloch. Although his career was cut short in 1944 (he was executed by the Germans as a member of the French Resistance), his writings have remained very much alive in subsequent years as historians have striven to expand the horizons of their discipline. Comparative history was the theme of the convention held by the American Historical Association in December 1978, and Bloch's name was frequently invoked by the participants in its programs. Yet the discussions at San Francisco revealed some ambiguity among scholars about the method and its potentialities. Consider, for example, the titles of some key sessions: "Can 'Comparative History' Be Defined?"; "How Has 'Comparative History' Been Practiced?"; and "Is There an Interdisciplinary Comparative Method?" Questions were more in evidence than answers. Some of the ambiguity stems from Bloch's own misconceptions about the model from which he derived his ideas on the comparative method—the model of historical linguistics.

IN HIS FAMOUS ARTICLE, "Pour une histoire comparée des sociétés européennes," Marc Bloch asserted that of all of the social scientists only the linguists had ac-

<sup>1</sup> Ferdinand de Saussure (1857–1913), *Course in General Linguistics*, ed. Charles Bally and Albert Sechehaye, trans. Wade Baskin (New York, 1966), 7. The original French edition, *Cours de linguistique générale* (1916) was compiled from student notes. For a recent criticism of structural linguistics, especially the tradition associated with Saussure, see Jacques Derrida, "Linguistics and Grammatology," in his *Of Grammatology*, trans. Gayatri Chakravorty Spivak (Baltimore, 1976), 27–73. Also see René Thom, "Topologie et linguistique," in André Haefliger and Raghavan Narasimhan, eds., *Essays on Topology and Related Topics: Mémoires dédiés à Georges de*

curately differentiated the techniques involved in comparison.<sup>2</sup> True comparison, he declared, entails "two widely different intellectual processes."<sup>3</sup> Bloch derived these two processes, which we will call "universal" and "historical," from the French linguist Antoine Meillet (1866–1936). In "The Definition of the Comparative Method," Meillet wrote, "There are two different ways of practicing comparison: one can compare in order to draw from comparison either *universal laws* or *historical information*. These two types of comparison, equally legitimate," he claimed, "differ absolutely."<sup>4</sup>

Meillet, one of the most respected and prolific scholars in comparative Indo-European linguistics during the late nineteenth and early twentieth centuries, published monographs on the grammars of the Greek, Latin, Germanic, Iranian, Old Slavonic, and, most notably, Armenian languages. In addition, he produced in 1903 a theoretical tract on the comparative study of all known Indo-European languages, *Introduction à l'étude comparative des langues indo-européennes*. A pupil of Ferdinand de Saussure, he was appointed professor of comparative Indo-European grammar at the Collège de France in 1906.<sup>5</sup> His interests included sociology as well as linguistics; beginning in 1901, he collaborated with Émile Durkheim on the journal *L'Année Sociologique*.<sup>6</sup>

Bloch relied upon Meillet's *La méthode comparative en linguistique historique*, published in 1925, for his decision to follow the historical rather than the universal linguists.<sup>7</sup> He described the two methods, or "processes," as follows:

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Rham (Berlin, 1970), 226–27. For a convincing argument as to why historians should acquaint themselves with linguistics, see Nancy S. Struever, "The Study of Language and the Study of History," *Journal of Interdisciplinary History*, 4 (1974): 401–15.

<sup>2</sup> Bloch, "Pour une histoire comparée des sociétés européennes," paper delivered at the Sixth International Congress of Historical Sciences, held in Oslo, August 1928, and printed in *Revue de synthèse historique*, 46 (1928): 15–50. There are two English versions: "A Contribution Towards a Comparative History of European Societies," trans. J. E. Anderson, in Bloch, *Land and Work in Mediaeval Europe: Selected Papers by Marc Bloch* (New York, 1969), 44–81; and "Toward a Comparative History of European Societies," trans. Jelle C. Riemersma, in Frederic C. Lane and Jelle C. Riemersma, eds., *Enterprise and Secular Change: Readings in Economic History* (Homewood, Ill., 1953), 494–521. Anderson's translation in *Land and Work in Mediaeval Europe* contains Bloch's original footnotes, and for that reason is generally more useful for the student. The Lane-Riemersma edition in *Enterprise and Secular Change*, however, has an excellent introduction demonstrating the influence of Émile Durkheim on Bloch's thought, and the editors have also provided the reader with a concise outline of Bloch's paper; "Toward a Comparative History," trans. Riemersma, 489–93, 494.

<sup>3</sup> Bloch, "Towards a Comparative History," trans. Anderson, 45, but see "Toward a Comparative History," trans. Riemersma, 495 ("Pour une histoire comparée," 16).

<sup>4</sup> Meillet, *The Comparative Method in Historical Linguistics*, trans. Gordon B. Ford, Jr. (Paris, 1967), 13 (italics added). We have labeled Type I "universal" and Type II "historical" because Bloch clearly relied upon Meillet for his use of two types of comparative analysis, and Meillet used these two terms, "universal" and "historical." It seemed to us, moreover, that a descriptive label, in addition to "Type I" and "Type II," would aid the reader in following the argument throughout the article. At present, most linguists would not find the term "universal" appropriate; they prefer "typological linguistics," although they do have occasion to use the term "universal grammar." See Joseph H. Greenberg, *Language Universals, with Special Reference to Feature Hierarchies* (The Hague, 1966), *Language Typology: A Historical and Analytic Overview* (The Hague, 1974), and, especially, *Universals of Human Language*, 4 vols. (Stanford, 1978).

<sup>5</sup> For a full account of Meillet's achievements, see the obituary by Joseph Vendryes, "Antoine Meillet," in Thomas A. Sebeok, ed., *Portraits of Linguists: A Biographical Source Book for the History of Western Linguistics, 1746–1963*, 2 (Bloomington, Ind., 1966): 201–40. For Saussure's influence on Meillet, especially see *ibid.*, 215–16, 217.

<sup>6</sup> Alf Sommerfelt, "Antoine Meillet, the Scholar and the Man," in Sebeok, *Portraits of Linguists*, 241–49; for Meillet's connection with Durkheim, see *ibid.*, 245. Also see Vendryes, "Antoine Meillet," *ibid.*, 223–24.

<sup>7</sup> Bloch, "Towards a Comparative History," trans. Anderson, 76 n. 3 ("Pour une histoire comparée," 16 n. 1).

*Type I (Universal Comparison):* "The historian selects some societies so widely separated in time and space that any analogies observed between them with respect to such and such phenomena can obviously not be explained either by mutual influence or by a common origin."<sup>8</sup>

*Type II (Historical Comparison):* "This is to make a parallel study of societies that are at once neighbouring and contemporary, exercising a constant mutual influence, exposed throughout their development to the action of the same broad causes just because they are close and contemporaneous, and owing their existence in part at least to a common origin."<sup>9</sup> In history proper, this is the equivalent of the historical study of languages (for example, Indo-European languages); whereas comparative history in the broad sense would more or less correspond to linguistics in general. In both history and language, it appears true that of these two comparative methods the one with the more limited horizon is also the richer in results. Because it is more capable of rigorous classification, and more critical about the objects it compares, it may hope to reach conclusions of fact that are less hypothetical and more precise. This at any rate is what I hope to be able to show; for it is certainly this method that I intend to elaborate."<sup>10</sup>

As an example of Type I comparison Bloch used Sir James Frazer's study of the cult of Diana in Roman times, which at Lake Nemi involved the ritual murder of the priest officiating at the temple. Frazer sought out similar rites among "barbarous" peoples at other places and times with a view to establishing a common psychological trait that would make plausible the religious rite at Nemi.<sup>11</sup>

As described by Bloch and illustrated by his choice of Frazer's example, Type I need not involve any genetic relationship among the things compared. From the point of view of the historical linguist, this sort of comparison would be virtually profitless. One can compare the Japanese and English languages, for instance, and find borrowings dating from fairly recent times that would reflect cultural interpenetration, but a comparison in the ultimate sense would yield negative results since the two languages do not have a common origin.<sup>12</sup> What

<sup>8</sup> Bloch, "Towards a Comparative History," trans. Anderson, 46 ("Pour une histoire comparée," 17).

<sup>9</sup> Bloch's phrase here, "in part at least to a common origin" ("partiellement du moins, à une origine commune"), seems unnecessarily qualified considering that a common origin is a basic assumption in historical linguistics. See Paul Thieme, "The Comparative Method for Reconstruction in Linguistics," in Dell Hymes, ed., *Language in Culture and Society: A Reader in Linguistics and Anthropology* (New York, 1964), 585-98; and the references in note 12, below.

<sup>10</sup> Bloch, "Towards a Comparative History," trans. Anderson, 47-48 ("Pour une histoire comparée," 19-20).

<sup>11</sup> Bloch, "Towards a Comparative History," trans. Anderson, 46 ("Pour une histoire comparée," 18-19).

<sup>12</sup> See Meillet, *The Comparative Method in Historical Linguistics*, 19; and Leonard Bloomfield, *Language* (New York, 1933), chap. 18: "The Comparative Method," 297-320. For a more recent statement, see Henry M. Hoenigswald, *Language Change and Linguistic Reconstruction* (Chicago, 1965), 68-71, 119-43. We are indebted to Henry Hoenigswald for his helpful suggestions and the references he placed at our disposal. Also see Thieme, "The Comparative Method for Reconstruction in Linguistics," 585-98.

Frazer sought was a common human substratum or deep structure of mentality in the Chomskyian sense—in short, a universal trait with world-wide application.<sup>13</sup> The search for language universals is a valid one, but the significance of the few that have been identified is questionable. In spite of their successes, modern linguists have significant problems to settle before their methodology can be recommended without reservation. Not too long ago, W. V. Quine criticized Noam Chomsky's lack of "explicitness of criteria and awareness of method" in crediting native speakers with a "precise sense of grammaticality." "The problem of evidence for a linguistic universal," Quine concluded, "is insufficiently appreciated."<sup>14</sup>

Bloch rejected universal comparisons of Type I in favor of historical comparisons of Type II. In the article of 1928 on "histoire comparée," he wrote, "I propose to compare the various European societies—especially in Eastern and Central Europe—societies that are contemporary, that live close to one another, and that go back if not to one common origin, at any rate to several."<sup>15</sup> If by "Eastern and Central Europe" Bloch meant the German, Slavic, and Hungarian societies, he proposed to study two groups that descended from a common linguistic ancestor (the Germans and the Slavs) and one group (the Hungarians) that has had a separate linguistic history. The German and Slavic languages have a "common origin," Proto-Indo-European, whereas Hungarian belongs to an entirely unrelated linguistic family, Finno-Ugrian. (There is, however, no *necessary* connection between language and ethnic origin.<sup>16</sup>)

Bloch's research proposal was of a sort familiar to comparative linguists (that is, those whose methods are Type II), accounting for similarities between the Slavs and the Germans on the basis of a genetic relationship. As spoken by families long resident in the area adjacent to Austria, Hungarian would doubtless show evidence of German borrowings, and similarly Hungarian institutions would be expected to reflect Austrian practices and forms. One might assume that in this case Bloch would search out what was native to Hungarian soil and what was merely borrowed from German-speaking Austrians. In studying the earliest institutions of German- and Slavic-speaking areas, Bloch would presumably find that some similarities could be accounted for by a common origin, since the speakers of Slavic and German were at one time members of the same tribe—the Indo-Europeans. This assumption is based upon the structuralists' theory that all institutions behave as languages do.<sup>17</sup>

<sup>13</sup> Noam Chomsky effected a revolution in linguistics with the publication of his *Syntactic Structures* in 1957. For the nature and significance of his work, see Gilbert Harman, ed., *On Noam Chomsky: Critical Essays, Modern Studies in Philosophy*, gen. ed. Amelie Oksenberg Rorty (Garden City, N.Y., 1974).

<sup>14</sup> Quine, "Methodological Reflections on Current Linguistic Theory," in Harman, *On Noam Chomsky*, 104–17, esp. 108–09.

<sup>15</sup> Bloch, "Towards a Comparative History," trans. Anderson, 48 ("Pour une histoire comparée," 20).

<sup>16</sup> For a forceful corrective to the older historiography, which tended to confuse language, race, and nation and to oversimplify East-West relations, see Frantisek Graus *et al.*, *Eastern and Western Europe in the Middle Ages*, History of European Civilization Library, gen. ed. Geoffrey Barraclough (London, 1970). In his Introduction, Barraclough cited Ranke and pinpointed the use of linguistic divisions in the study of history as one of the main reasons for the assumed superiority of Western over Eastern Europe and the stigmatization of the Slavs as "silent spectators"; *ibid.*, 7. Also see *ibid.*, 22–25.

<sup>17</sup> Michael Lane, ed., *Introduction to Structuralism* (New York, 1970), 18. Also see Spivak, Preface to Derrida, *Of Grammatology*, liv–lxvii, and Derrida, *Of Grammatology*, chap. 2: "Linguistics and Grammatology," 27–73.

Although Bloch planned to study societies, not languages, Meillet's techniques were appropriate to his purpose. But, when he shifted his discussion to the comparative study of feudalism, he demonstrated the limits of his understanding of those techniques:

If I am studying the manorial system in the Limousin, I shall be continually impelled to consider setting side by side information drawn from other manors; in common or garden language, I shall be comparing them. But I shall not consider myself to be engaged in what is technically called "comparative" history, for I shall be taking the different objects studied from a cross-section of a single society in which, looked at as a whole, there is a considerable degree of unity. In practice, it has become customary to reserve the term "comparative history" almost entirely for the comparative examination of phenomena that have taken place on different sides of a State, or national, frontier.<sup>18</sup>

Contemporary linguists did not differentiate so sharply between local and international units of comparison. Dialect geographers were building their atlases exactly as Bloch described for his survey of manors in the Limousin.<sup>19</sup> One of the most important of these was Jules Gilliéron (1854–1926), whose work Meillet greatly admired.<sup>20</sup> Dialect geography was quite as respectable and just as important on the small-scale level of, say, a canton in Switzerland as on the large-scale level of Indo-European, a linguistic territory that even in medieval times stretched from Iceland to the Indian subcontinent. Some linguists have argued that the fundamental work comes at the finest level of the grid and that the comprehensive comparison of dialects and then of languages can only be undertaken with a degree of accuracy and expectation of success when the meticulous work of collecting local speech patterns has been accomplished.<sup>21</sup> In actual prac-

<sup>18</sup> Bloch, "Towards a Comparative History," trans. Anderson, 45–46 ("Pour une histoire comparée," 17).

<sup>19</sup> For a discussion of the problem of differentiating dialects from languages, see Henry M. Hoenigswald, "Criteria for the Subgrouping of Languages," in Henrik Birnbaum and Jaan Puhvel, eds., *Ancient Indo-European Dialects: Proceedings of the Conference on Indo-European Linguistics Held at the University of California, Los Angeles, April 25–27, 1963* (Berkeley and Los Angeles, 1966), 1–12. Furthermore, Hoenigswald pointed out that "the criteria of subgrouping have constituted one of the most frequently discussed topics in comparative linguistics from the middle of the last century to this day"; *ibid.*, 1 n. 1.

<sup>20</sup> See Mario Roques, "Jules Gilliéron," in Sebeok, *Portraits of Linguists*, 65–73. Meillet's enthusiasm for dialect geography in general and for Gilliéron in particular can be found in the words of Joseph Vendryes, Meillet's pupil and colleague: "Meillet a été des premiers ... à comprendre ce qu'il y avait de génial dans les idées de Gilliéron. Il a suivi et encouragé ses travaux; il en a lui-même profité pour perfectionner sa propre méthode. Il s'en est inspiré en effet pour écrire son article sur *les effets de l'homonymie dans les langues indo-européennes* ... et l'influence s'en manifeste aussi dans son livre sur les *Dialectes indo-européens*"; "Antoine Meillet," 220. Although Gilliéron was devoted to comparisons on the small scale, Meillet used these principles to study the whole corpus of Indo-European.

<sup>21</sup> Robert A. Hall, Jr., "The Reconstruction of Proto-Romance," *Language*, 26 (1950): 6–27. Hall recommended a more accurate description of local areas before the work of comparison should begin in earnest, and this sort of description has had strong advocates among historians. See Pierre Goubert, "Local History," *Daedalus: Journal of the American Academy of Arts and Sciences*, 100 (1971): 113–27, reprinted in *Historical Studies Today*, Proceedings of the American Academy of Arts and Sciences (New York, 1972), 300–14. Goubert cited a number of examples demonstrating that regional and provincial studies in France had successfully challenged historical generalizations. One example must suffice in this context: "The growth of the seignorial system (which the Marxists call feudal) in France seemed to be quite common; an edict by Louis XIV was sometimes cited to show how the last allodial tenures fared under that king. R. Boutruche, in his study on allodial tenure in the Bordeaux region, has drawn our attention to the fact that completely free lands and peasants sometimes survived the Middle Ages. In his book on the Lower Auvergne, Abel Poitrineau shows, on the basis of notarial documents, that the proportion of allodial tenures sometimes approached 50 per cent in many rural communities of eighteenth-century Auvergne. This phenomenon will probably be found to have been common in other parts of the country as well (mainly in central, eastern, and southern France). The result of these many



tice Bloch's comparisons of local manors and of much larger units were equally impressive. Bloch made it clear that he did not consider comparative history to be mainly a matter of comparing states, an approach that he called "a gross simplification."<sup>22</sup> Yet he did not stay within the Type II comparison, either in practice or in statements of theory. Once he had distinguished universal comparison as practiced by James Frazer from the historical comparison of Meillet, he went on to develop his own form of comparison, which differed markedly from that of Type II. Later in his career he appeared to come back to Type II, but he apparently never made his preference clear. The attraction of Type II comparison—that of historical linguists since the Neogrammarians of Leipzig in the 1870s—proved irresistible to Bloch because of its epoch-making results, but he did not practice it consistently, ostensibly because he did not compare languages or dialects but other social entities.<sup>23</sup>

Bloch's lack of rigor in following the linguistic model of Meillet can be explained by his belief that "the history of social organisation is . . . a much more difficult problem" than comparable studies in linguistics; by contrast he called the "problem of linguistic affiliations" relatively simple.<sup>24</sup> And he cited Meillet on the sharp distinction between language and history:

"Up to now," writes Meillet, "no case has been discovered giving cause for the belief that the morphological system of a given language is the result of the intermingling of the morphologies of two distinct languages. In all cases so far observed, a language presents a continuous tradition," whether this tradition be "of the current type—the transmission of the language from the adults to the children"—or whether it arises "from a change of language." But let us suppose that at a given moment examples are discovered of this phenomenon so far unknown—"real mixtures" between languages. When that happens (I am still quoting M. Meillet) "linguistics will have to devise new methods." Now the historian of societies finds that the facts themselves impose upon him this formidable hypothesis of "mixtures," which, if realised in the linguistic field, would be such a disturbing influence in that humane science which is rightly most self-confident.<sup>25</sup>

Meillet insisted that the success of the comparative method presupposed a common origin. Furthermore, he said that the method also depended upon the "arbitrary character of the sign."<sup>26</sup> Bloch did not discuss this second, fundamental

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researches is the inescapable (but unanticipated) conclusion that a part of France during the Ancien Régime (and, by definition, the Middle Ages as well) did not undergo the signorial system." *Ibid.*, 118. One can cite examples from Bloch's own writings to support this contention—for example, the need for detailed studies in the context of his definition of "European feudalism"; Bloch, *Feudal Society*, vol. 2: *Social Classes and Political Organization*, trans. L. A. Manyon (1961; Chicago, 1964), 446. For Bloch's recognition of the survival of allodial lands, see his "The Rise of Dependent Cultivation and Seignorial Institutions," in M. M. Postan, ed., *The Cambridge Economic History of Europe*, vol. 1: *The Agrarian Life of the Middle Ages* (2d ed., Cambridge, 1966), 270.

<sup>22</sup> Bloch, "Towards a Comparative History," trans. Anderson, 46 ("Pour une histoire comparée," 17: "c'est là une simplification *un peu grosse*" [italics added]).

<sup>23</sup> For a summary of the history of linguistic development before and after the "revolution" of the Neogrammarians at Leipzig, see Holger Pedersen, *The Discovery of Language: Linguistic Science in the Nineteenth Century*, trans. John Webster Spargo (Bloomington, Ind., 1962), chap. 7: "The Methods of Comparative Linguistics: A Survey of Their Development," 240–310.

<sup>24</sup> Bloch, "Towards a Comparative History," trans. Anderson, 68 ("Pour une histoire comparée," 41).

<sup>25</sup> Bloch, "Towards a Comparative History," trans. Anderson, 68 ("Pour une histoire comparée," 41). Bloch drew here on Meillet's *La Méthode comparative en linguistique historique*, 82, 83; see Bloch, "Towards a Comparative History," trans. Anderson, 80 n. 29 ("Pour une histoire comparée," 42 n. 1).

<sup>26</sup> Meillet, *The Comparative Method in Historical Linguistics*, 14: "Only the totally arbitrary character of the sign makes possible the historical and comparative method which will be studied here."

aspect of the subject in this context.<sup>27</sup> If Meillet's common origin and the "arbitrary character of the sign" are both essential to the proper exercise of Type II comparison, then it appears incongruous for Bloch to have endorsed Meillet's method so strongly as a proper model for the historian. To assert, as Bloch did, that the subject matter of language affiliation is endowed with simplicity is not justified.<sup>28</sup> An examination of the studies of those linguists who have dealt with the problems of comparison, including how change takes place, reveals that there is nothing simple about it. Whereas a morpheme may be a *smaller* unit than an institution, the complexity of interpretation is not necessarily less. Whoever peruses the various types of phonetic, analogic, and semantic change in Leonard Bloomfield's *Language* or the intricate formulae in Henry M. Hoenigswald's *Language Change and Linguistic Reconstruction* will not readily conclude that simplicity inheres in the study of "linguistic affiliations."

WHAT OUGHT TO BE COMPARED IN ANY STUDY that claims to follow the method used in comparative historical linguistics is *all and only* the phenomena in a related group—for example, the Romance languages. One would not make a comparison of French and Italian, or Spanish and Rumanian, and hope to reconstruct an accurate version of Proto-Romance. Only a study of all of these languages, integrating the background data on the dialects of each, would begin to yield important results.<sup>29</sup>

A classic example of the fallacy of simply comparing two languages was provided by Ferdinand de Saussure in his *Course in General Linguistics*, in which he cited certain forms of Latin *genus*, Greek *génos*, and Sanskrit *ganas*, all of which mean "race" (see Table 1).<sup>30</sup> These words are clearly cognate, for they correspond to a high degree in both phonemic components and in semantic value.

<sup>27</sup> In a short article, Bloch showed that he was aware of this concept in linguistics, but he did not explain how it might be applied in comparative history: "Enfin—M. Meillet, entre autres, a fortement insisté sur ce dernier trait—les notations du langage sont, en règle générale, purement arbitraires; il n'y a pas de raison intrinsèque pour que 'beau' exprime la beauté et 'laid' la laideur. Le nombre des combinaisons de cette espèce étant, en pratique, à peu près illimité, il apparaît clairement que les concordances linguistiques ne peuvent que très rarement s'expliquer par le hasard; elles doivent, pour la plupart, être interprétées comme la preuve, soit d'influences réciproques, soit d'une origine commune; or c'est, nous le verrons, surtout à retracer les filiations des langues que s'est, pendant longtemps, appliquée la linguistique comparée." Bloch, "Comparaison," *Revue de synthèse historique*, 49 (1930): 32–33.

<sup>28</sup> Bloch seemed ambivalent on the point of simplicity, for elsewhere he said, "We must recognise that the facts of agrarian life are no tidier than those of language: just as there are always certain forms and usages which straddle dialect boundaries, so there are no geographical areas whose limits are exactly coterminous with one particular set of agrarian forms and techniques." See Bloch, *French Rural History: An Essay on Its Basic Characteristics*, trans. Janet Sondheimer (Berkeley and Los Angeles, 1966), 62. The French edition, *Les Caractères originaux de l'histoire rurale française* (1931), originated in a series of lectures Bloch delivered at the Institute for the Comparative Study of Civilisations in Oslo in 1929 and recast into book form.

<sup>29</sup> For an eloquent case for the necessity of strictly applying the comparative method and the pitfalls of using it superficially, see Hall, "The Reconstruction of Proto-Romance," 6–27.

<sup>30</sup> In his biography of Saussure, Jonathan Culler has reproduced this example, and, where Saussure gave the locative plural (instead of the nominative plural) in Sanskrit as *ganasu*, Culler has substituted *ganassu*; Culler, *Ferdinand de Saussure* (Harmondsworth, Eng., 1977), 60. Culler may have taken this spelling from the variorum edition of Saussure's work: Saussure, *Cours de linguistique générale*, ed. Rudolf Engler (Wiesbaden, 1967), 5, 6. The form *ganassu* should have been supplied with an asterisk since it is a reconstruction. The attested locative plural is *ganahsu*. For a complete paradigm of this type of noun in Sanskrit, see William Dwight Whitney, *Sanskrit Grammar* (Cambridge, Mass., 1955), 154–55.

TABLE 1  
Latin, Greek, and Sanskrit Cognates for “Race”

<i>Case</i>	<i>Latin</i>	<i>Homeric Greek</i>	<i>Sanskrit</i>
SINGULAR			
NOMINATIVE	genus	génos	ḡanas
GENITIVE	generis	géneos	ḡanasas
DATIVE	generi	génei	
LOCATIVE			ḡanasi
PLURAL			
NOMINATIVE	genera	génea	ḡanāṅsi
GENITIVE	generum	géneōn	ḡanasām

SOURCE: Ferdinand de Saussure, *Course in General Linguistics*, trans. Wade Baskin (New York, 1966), 2.

NOTE: We have supplied the nominative plural of Sanskrit *ḡanas* (*ḡanāṅsi*) where Saussure gave the locative, *ḡanasu*, because he cited the Latin and Greek in their nominative forms (*genera* and *génea*). Saussure probably omitted the Sanskrit nominative plural because it contains an intrusive nasal. Actually, he cited the locative plural incorrectly: it should be *ḡanaḥsu*. (Perhaps we should blame his students for their inattention rather than the master; see note 1, above.)

Yet we would be misled about their morphological development if we were to examine the Greek and Latin evidence only and omit the Sanskrit. In the genitive singular, for example, the Latin has an interior /r/, whereas the Greek has a zero phoneme. With only these two languages before us, we would assume either that the Greek had lost an original /r/ or that the Latin had infixing one. Neither of these phenomena is attested for the phonological history of these languages. But, when we consider the Sanskrit genitive singular, *ḡanasas*, the puzzle is solved: the /s/ is an Indo-European retention of the stem consonant, which was rhotacized in Latin—that is, the /s/ became an /r/—and was lost in Greek. Both of these phenomena—rhotacism of intervocalic /s/ in Latin and loss of intervocalic /s/ in Greek—are attested, and numerous examples of each prove that the solution to the problem lies in the assumption of an original Indo-European /s/ where Latin has /r/ and Greek has zero.

Such cases are the norm, not the exception, in the reconstruction of Indo-European dialect interrelations. Another example is “Grassmann’s Law.”<sup>31</sup> Taking the same word discussed above, the Germanic equivalent has an initial /k/ instead of /g/, as, for example, the English *kin*. Where Greek and Latin have a /g/ (that is, a voiced stop), English has a voiceless stop. Proto-Germanic, the ancestor of English, underwent a change called the First Sound Shift, in which all of the voiced stops, not just /g/, were unvoiced: /g/ > /k/; /d/ > /t/; and /b/

<sup>31</sup> See Bloomfield, *Language*, 349–51. Hermann Grassmann (1809–77) published his findings in 1862; *ibid.*, 349.

> /p/.<sup>32</sup> Similarly, the voiced aspirated stops became deaspirated stops: /bh/ > /b/; /dh/ > /d/; and /gh/ > /g/. Again, Sanskrit provides the evidence: Sanskrit *bhrātā* (“brother”), where Sanskrit retains the PIE aspirate, and English *brother*, where the aspiration is absent; the Sanskrit root *dhā-* (“place,” “put”) and English *do*; and Sanskrit *grabh* (“grasp”) and English *grab*. The evidence from Greek and Latin, as well as from Germanic and Sanskrit, is overwhelming that indeed the aspirated voiced stops of Proto-Indo-European lost their aspiration in the Germanic group, becoming simple voiced stops.

Yet certain words that seem to be cognate defy this rule, which was a part of Grimm’s Law describing the First or Germanic Sound Shift. Hermann Grassmann derived a corollary, which he could not have done had he restricted himself to the study of two languages. The Neogrammarians of Leipzig in the 1870s assumed that language change was regular in the highest degree, and, if a portion of the linguistic evidence did not fit the formula, this deficiency was not a reason to ignore the evidence or to label it anomalous. A way had to be found to restate the rule so that all examples would be subsumed under it. What Grassmann did was to look at the cases of Germanic, simple voiced stop consonants that would all theoretically go back to an original Indo-European voiced aspirated stop, like *do* in relation to *dhā-*. In the cases in which English /d/ was paralleled not by /dh/ but by /d/, he found that all of them occurred in the context of another aspirated voiced stop. For example, English *bid* is cognate with Sanskrit *bodh-* (“make known”) instead of *\*bhodh-*. The initial plain, unaspirated stop should be an aspirate in Sanskrit—/bh/—just as the final consonant of English *bid* is paralleled by the final consonant /dh/ in the Sanskrit root. Since there is an observable rule in Sanskrit that two aspirates cannot stand in close proximity, Grassmann suggested that the initial consonant had indeed been aspirated originally—a /bh/—and that Sanskrit had deaspirated it. Greek has a similar rule, or a parallel pattern of phonemic change: two aspirates cannot stand in this way; one of them is deaspirated. Thus, Grassmann reformulated a part of Grimm’s Law to cover these cases: the Indo-European voiced aspirated stops become deaspirated in Sanskrit and Greek, if they stand in the syllable adjacent to another aspirated stop. This theory cleared up a whole group of mystifying cognates in which the Germanic evidence seemed to conflict with the Sanskrit and the Greek.

If Grassmann had confined himself to Sanskrit and Greek alone, he might not have discovered this important exception to Grimm’s Law. Germanic evidence, exemplified by English among others, retained a simpler reflex of the original development of these phonemes than did Sanskrit or Greek, both of which were generally thought to preserve more of the ancient features of the parent language. Thus, the comparison of only two of a related group of languages is unnecessarily restrictive and will possibly lead to fallacious conclusions.

<sup>32</sup> The symbol “>” means “becomes” or “results in.” The First or Germanic Sound Shift is also known as “Grimm’s Law,” after Jakob Grimm (1787–1863), who published his statement on consonant relations in 1822, in the second edition of his *Deutsche Grammatik*; Bloomfield, *Language*, 14.

In his article on “histoire comparée” in 1928, Bloch espoused the Type II method of the linguists in preference to the Type I search for universals, but he did not show that he fully understood Type II. For example, he did not make it clear that *all* societies with a similar institution must be studied if there is reason to suspect a genetic relationship among them. Instead, he spoke of studying “neighbouring” societies. To be sure, neighboring societies would doubtless show cultural similarities, but it might be possible to find a society at some distance that had a similar institution—for example, the *rājā* of ancient India, the *rex* of ancient Rome, and the *\*rīgs* of Celtic-speaking Europe. Since these words are cognate—that is, they derive from a single Proto-Indo-European root—comparing them as royal institutions would be plausible, even though, at the present time, the cultures that inherited these terms have spread apart and are no longer “neighbouring” in the geographical sense.

Bloch said, moreover, that he wished to trace the institutions under consideration “if not to one common origin, at any rate to several.” The Neogrammarians of Leipzig, who are credited with the major breakthrough in comparative historical linguistics in the nineteenth century, operated upon the assumption that related forms should lead back to a single form, not to several. Bloch had absorbed their premise of the goal of reconstruction, but he did not appear to insist upon the assumption of a single origin.<sup>33</sup>

Finally, in Bloch’s own work there are cases in which he compared institutions in only two societies—for example, in his study of the *ministeriales* in France and Germany and of the so-called *rois thaumaturges* in France and England.<sup>34</sup> It could be argued that in each of these works the greater part of the evidence was confined to two societies and, therefore, Bloch was indeed using “all and only” the pertinent data. Most linguistic comparisons, however, involve a great many more groups, whether at the level of dialect or of language. One wonders if these fine works by Bloch—one an article, the other a book—did not perhaps set a precedent for his successors to confine their studies to two societies, rather than “all and only.”

Marc Bloch’s remarks on the methodology of comparative history lack precision and coherence. One can only conclude that his conflicting statements derive from his admiration for linguistic research and a regrettable lack of time to study its full implications. So great was Bloch’s influence on subsequent scholarship that the flaws in his statements on the value of the linguistic model have

<sup>33</sup> That the goal of reconstruction was a *sine qua non* of the Neogrammarians, as opposed to the less rigorous comparison of preceding linguists at Leipzig and elsewhere, is well known and can be established by reference to any number of scholars who endorsed it—among them, Saussure and Meillet. One prominent American who studied at Leipzig in the late 1880s and early 1890s was Carl Darling Buck (1866–1955). In the preface to his monograph devoted to the vowels of Oscan, an Italic dialect, he insisted that reconstruction, not mere comparison, was the final goal of the historical linguist. Buck was one of the first faculty members of the then newly founded University of Chicago, and his influence was felt by many later scholars in the United States. See George S. Lane, “Carl Darling Buck,” in Sebeok, *Portraits of Linguists*, 266–77, esp. 270.

<sup>34</sup> Bloch, “A Problem in Comparative History: The Administrative Classes in France and in Germany,” trans. J. E. Anderson, in Bloch, *Land and Work in Mediaeval Europe*, 82–123, and *The Royal Touch: Sacred Monarchy and Scrofula in England and France*, trans. J. E. Anderson (London, 1973).



been repeated in the work of his disciples.<sup>35</sup> A methodological tradition has developed that needs to be critically examined.

IN THE EDITORIAL PREFACE TO THE FIRST ISSUE of the distinguished journal *Comparative Studies in Society and History*, Sylvia L. Thrupp applauded the historical linguists for their methodology and then advised future contributors to "select whatever approach to comparative study" that appeared "most appropriate to their purposes. Some may prefer the approach through testing or application of theory which has often been identified with 'the comparative method.' Where theory is precise, and there is a limited body of data of such a character that all of it can be rigorously checked from the point of view of each competing theory, this is undeniably the best plan. Its success in historical linguistics triumphantly proves the point. Very few of the recurrent cultural problems, however, present themselves under these conditions." Yet the "triumph" of the historical linguists was not owed to the simplicity or the uniformity of their subject matter but to the precision of the method they developed. Thrupp went on to suggest two other approaches: she rejected the "purely empirical" as leaving too free a rein to "personal idiosyncrasy," but she recommended "a third or mixed" approach as probably appropriate for the majority of contributors, "drawing to some extent on theory as a means of control over empirical observation."<sup>36</sup>

In these instructions, Sylvia Thrupp defined neither "theory" nor "the comparative method," leaving these abstractions to the contributor's imagination. It need not be assumed, however, that the methods worked out by linguists are inappropriate for social scientists because of the intractability of the subject matter. Henry Sumner Maine (1822–88) was so impressed by the work of comparative philologists that he put into practice some of their principles in his studies of ancient jurisprudence, works that are still respected by both historians and anthropologists.<sup>37</sup> Claude Lévi-Strauss took the theory of phonemic struc-

<sup>35</sup> J. Ambrose Raftis, "Marc Bloch's Comparative Method and the Rural History of Mediaeval England," *Mediaeval Studies*, 24 (1962): 349–68, esp. 351. Raftis observed that Bloch was "never able to leave us a complete study of his methodology for comparative history," yet he asserted that a methodology could be obtained by reading Bloch's articles in *Annales* during the 1930s. The overwhelming number of articles on Bloch have made a similar claim in order to give Bloch's inconsistencies a specious coherence.

<sup>36</sup> Thrupp, "Editorial," *Comparative Studies in Society and History: An International Quarterly*, 1 (1958): 2–3.

<sup>37</sup> For two of his principal works, see Maine, *Ancient Law: Its Connection with the Early History of Society and Its Relation to Modern Ideas* (London, 1861), and *Village-Communities in the East and West: Six Lectures Delivered at Oxford, to Which Are Added Other Lectures, Addresses, and Lectures* (New York, 1876). For an illuminating evaluation of Maine's work, see Kenneth E. Bock, "Comparison of Histories: The Contribution of Henry Maine," *Comparative Studies in Society and History*, 16 (1974): 232–62. Bock disproved the commonly held notion of Maine as an "evolutionist" by demonstrating his admiration for the comparative philologist Max Müller, who was Maine's contemporary (1823–1900). Bock did not, however, point out that Müller's work was erroneous, although he did refer the reader to an important paper by Carl Darling Buck for "an informative discussion of the implications of comparative grammar for other studies." For Buck's article, see his "The Relations of Comparative Grammar to Other Branches of Learning," in Howard J. Rogers, ed., *History of Language, History of Literature, History of Art*, vol. 3 of *Congress of Arts and Science: Universal Exposition, St. Louis, 1904* (Boston, 1906), 32–52. Maine might well have contributed more to the integration of linguistic theories into the field of ancient history if he had had at his disposal the reliable work of Buck and other students of the Neogrammarians rather than only the dubious work of Müller, who was considered a popularizer at best; unfortunately, Maine's career barely overlapped the introduction of superior methods by the linguists at Leipzig.

ture and applied it to kinship relations, thereby achieving distinction in the field of ethnology.<sup>38</sup> More recently, linguistic techniques have been applied by a group of French psychoanalysts led by Jacques Lacan, who had come to the conclusion that the structure of the unconscious was the structure of language.<sup>39</sup>

J. G. A. Pocock has demonstrated a profound appreciation of linguistic problems in the interpretation of historical data:

It needed only the step—which a historian should take instinctively—of viewing “language” as a product of history and as possessing history of its own, to reach the point where it could be seen, first, that the exploration of language might yield historical results, might produce second-order statements about languages used which would be historical statements; second, that this activity could be considered a historical agent, helping to produce changes in linguistic consciousness and so in the history of language-use itself.<sup>40</sup>

In his review of the first volume of *History and Theory*, Pocock declared, “Since the historian’s mode of thinking, his assumptions, his problems, possibly even his logic, vary from tradition to tradition, period to period, and society to society, he must cease to be a *confrère* whose problems we share and become a phenomenon whose location and behaviour we study; and this study may be undertaken as a branch of comparative history.”<sup>41</sup> Interestingly enough, although Pocock referred explicitly to linguistics and to comparative history, he did not make the leap that Lévi-Strauss was bold enough to try—namely, the application of a method used by the linguists to the subject matter of history. And again we are left uncertain as to what “comparative history” and its connection to linguistics really consist of. Pocock’s own practice of comparative history as reflected in “The Origins of Study of the Past: A Comparative Approach” owes nothing to the historical linguists, nor does he allude to them as the source of his inspiration.<sup>42</sup>

A recent study of Marc Bloch and the comparative method stresses the logic of comparatism, but its author, William H. Sewell, Jr., noted the following limitations on the application of this logic:

The most important limit of the comparative method is not that it can be applied only when we are trying to explain phenomena which transcend the boundary of any single social system, but that it aids us only in one step, and that the easiest and most mundane step, of the explanatory process. *The comparative method is a method, a set of rules which can be methodically and systematically applied in gathering and using evidence to test explanatory hypotheses.*

<sup>38</sup> Lévi-Strauss, “Language and the Analysis of Social Laws,” in his *Structural Anthropology*, trans. Claire Jacobson and Brooke Grundfest Schoepf (Garden City, N.Y., 1967), 54–65.

<sup>39</sup> Jan Miel, “Jacques Lacan and the Structure of the Unconscious,” in Jacques Ehrmann, ed., *Structuralism* (Garden City, N.Y., 1970), 94–101. Miel stressed the appropriateness of linguistics as a model for “the scientific study of the unconscious” and concluded this part of his discussion with the statement that “Lacan’s revolution in psychoanalysis has many affinities with the thought of Lévi-Strauss”; *ibid.*, 98–99. Psychiatry or psychoanalysis may not be classed as “social sciences,” but, since Bloch was interested in the subject, the lack of “social” with “science” seems irrelevant. For Bloch’s interest, see his address, “Technical Change as a Problem of Collective Psychology,” trans. J. E. Anderson, in Bloch, *Land and Work in Mediaeval Europe*, 124–35.

<sup>40</sup> Pocock, *Politics, Language, and Time: Essays on Political Thought and History* (New York, 1973), 12.

<sup>41</sup> Pocock, Review of *History and Theory*, in *Comparative Studies in Society and History*, 4 (1962): 535.

<sup>42</sup> Pocock, “The Origins of Study of the Past: A Comparative Approach,” *Comparative Studies in Society and History*, 4 (1962): 209–46.

*It does not supply us with explanations to be subjected to test: this is a task for the historical imagination.*<sup>43</sup>

Sewell's analysis of Bloch devolves upon the assumption that a coherent definition of "the comparative method" has been agreed upon. Unlike others,<sup>44</sup> however, Sewell demeaned the "method" as being useful at only the lowest level of the explanatory process. Yet his overall analysis of Bloch as a "hypothesis-tester," a label that would fit all scholars, does not lead us closer to what comparative historians can claim as a method. Sewell relied upon Bloch, and Bloch relied upon Meillet, whose major goal was *reconstruction*; but, since Bloch was not a consistent follower of Type II methodology, Sewell's statement that the comparative method helps us only at "the easiest and most mundane step" is readily understandable. The comparative method used by Type II linguists is indispensable at the lowest and highest (or smallest and largest) levels of comparison—none of them necessarily "easy." The counterpart of reconstruction for the working historian is the origin of a given institution, and "origin," like the word "cause," has become unpopular for various reasons. Bloch's own warning against the "idol of origins" is one of the best known.<sup>45</sup>

What attracted historians to linguists was the apparent success of the latter in inventing formulae that would account for human behavior and in predicating sources that were sometimes later confirmed by the discovery of texts. For example, in his doctoral dissertation Saussure studied the vowel patterns of Proto-Indo-European, from which he postulated the existence of hitherto unexplained entities, which he called the *coefficients sonantiques*.<sup>46</sup> These phonemes, which were barely perceptible in the extant remains of Indo-European, were later confirmed by the discovery of Hittite texts and their subsequent decipherment in 1915. Saussure "had discovered, by a purely formal analysis, what are now known as the laryngeals of Indo-European."<sup>47</sup> A review of the history of various theories surrounding the laryngeals should convince historians that they should not assume that their own discipline is more complex than linguistics, either in theory or in practice. Linguists, moreover, may claim that their theoretical statements have tended to be more congruent with their handling of the data than have those of comparative historians.

Finally, an intriguing feature of Sewell's article is its distinction between the "comparative perspective" and the "comparative method." The former calls for "viewing historical problems in a context broader than their particular social,

<sup>43</sup> Sewell, "Marc Bloch and the Logic of Comparative History," *History and Theory*, 6 (1967): 217 (author's italics).

<sup>44</sup> Ambrose Raftis, for example, concluded that there was a comparative "method" derivable from Bloch's articles; see note 35, above.

<sup>45</sup> Bloch, *The Historian's Craft*, trans. Peter Putnam (New York, 1953), 29.

<sup>46</sup> Edgar Polomé, "The Laryngeal Theory So Far: A Critical Bibliographical Survey," in Werner Winter, ed., *Evidence for Laryngeals* (The Hague, 1965), 9–78. Polomé quoted Saussure's study, *Mémoire sur le système primitif des voyelles dans les langues indo-européennes*, cited its date of publication as 1879; Jonathan Culler said it was 1878; but Meillet stated that, although it was actually published in December 1878, the title page shows the date as 1879; Polomé, "The Laryngeal Theory So Far," 10 n. 1; Culler, *Ferdinand de Saussure*, 67; and Meillet, "Notice," in Sebeok, *Portraits of Linguists*, 93–94.

<sup>47</sup> Culler, *Ferdinand de Saussure*, 68.

geographical, and temporal setting.”<sup>48</sup> Whereas the “comparative method” lends itself to “rules,” the “comparative perspective” supposedly leads to “insights.” Perhaps so; but, before adopting Sewell’s universalism, it would seem essential first to comprehend the misunderstanding of the “comparative method” that emanated from Bloch’s early work.<sup>49</sup>

AT THE END OF HIS CAREER, Marc Bloch actually attempted the Type II method. In the *Cambridge Economic History of Europe* he traced the medieval lord back to “rustic chieftains” who inhabited much of Western and Central Europe in pre-Roman times.<sup>50</sup> Here Bloch dealt directly with the problems of origins, and he seems to have used “all and only” the appropriate evidence. By so doing, he obviously resembled the linguist who searched for the hypothetical ancestor of the latter-day branches of Proto-Indo-European. “Our object being to inquire into the origins of the rural *seigneurie* in Western and Central Europe, our first task must necessarily be to form as clear an idea as possible of what it was like when fully developed. You cannot study embryology if you do not understand the grown animal.”<sup>51</sup> It is striking that Bloch’s “rustic chieftain” and the historical linguists’ Proto-Indo-European are both shadowy reconstructions pieced together from all available remains.<sup>52</sup> In *The Historian’s Craft*, however, Bloch expressed a strong aversion to the “Idol of Origins.” Here he argued that too often historians have used the origin of an institution as an explanation and analysis of its growth and development, thus unduly limiting the task of explaining the context, the relationships, and the structures of human institutions.<sup>53</sup>

In his *Feudal Society*, as in the *Cambridge Economic History of Europe*, there is evidence of Type II comparison, but at the end of *Feudal Society* there is a suggestion

<sup>48</sup> Sewell, “Marc Bloch and the Logic of Comparative History,” 218. Sewell’s distinction between a more rule-oriented “method” and a general approach or outlook is echoed in other works dealing with this problem; see, for example, Shmuel N. Eisenstadt, “Social Institutions: Comparative Study,” in David L. Sills, ed., *The International Encyclopedia of the Social Sciences*, 14 (New York, 1968): 421–29.

<sup>49</sup> For a damaging review of universal comparison in the field of anthropology, see E. E. Evans-Pritchard, “The Comparative Method in Social Anthropology,” in his *The Position of Women in Primitive Societies and Other Essays in Social Anthropology* (London, 1965), 13–36. He showed how various eminent anthropologists have forced their evidence into a Procrustean bed in order to follow what they assumed was the validity of universal comparison. “The statistical use of the comparative method and on a world-wide scale has been abandoned in this country and, it would appear, is obsolete in Holland. In England, however, the establishment of laws or universals, in the sense of propositions to which there are no exceptions, by comparative analysis continued to be advocated by [Alfred R.] Radcliffe-Brown, one of the most dedicated and influential teachers in our subject, though he did not use statistics, and his version of the comparative method was in practice mainly a return to the illustrative method. Indeed, I have to say, with regret, that [Andrew] Steinmetz would have stigmatized much of what he wrote as idle speculation, and [Herman J.] Nieboer would have regarded it as an example of what he strongly protested against, the capricious practice of some writers of thinking up some plausible explanation of some social phenomenon and then searching round for illustrations which seem to support it and neglecting the rest of the material relating to the topic under consideration.” *Ibid.*, 23.

<sup>50</sup> Bloch, “The Rise of Dependent Cultivation and Seigneurial Institutions,” 235–90. For the term “rustic chieftains,” see *ibid.*, 284. Also see *ibid.*, 290: “A very ancient structure of rural chiefdoms was the essential nucleus, and about it the centuries deposited their successive layers one by one.” And, once again, Bloch referred to Meillet; *ibid.*, 273.

<sup>51</sup> Bloch, “The Rise of Dependent Cultivation and Seigneurial Institutions,” 235.

<sup>52</sup> *Ibid.*, 237–38. Bloch again, however, muddled the waters by alluding to “comparative methods”; *ibid.*, 237 (italics added).

<sup>53</sup> Bloch, *The Historian’s Craft*, chap. 1, pt. 4: “The Idol of Origins,” 29–35.

of Type I—namely, a comparative study of institutions in the universal sense, which he did not endorse in 1928. Here Bloch defined “feudalism” in such a way that both Japan and Europe could be examined without regard to their possible historical links.<sup>54</sup> Hence, he still seems to be the father of comparative history, even though he did not define the method he recommended in 1928 (Type II) in such a way as to clarify it for the use of practicing historians. Did Bloch finally understand what he was advocating? In *Feudal Society* he wrote that the burden of comparative history was too great for one man but that fortunately useful groundwork had previously been laid. “The task is facilitated by the existence of excellent studies which already bear the hall-mark of the soundest comparative method.”<sup>55</sup> In 1928 he described only two methods, and in 1939 he iterated his endorsement of Type II; but in 1940, in *Feudal Society*, his reference to the “soundest comparative method” leaves us in limbo, for the use of the superlative would imply that more than two types of comparison were being judged.<sup>56</sup> In any case, Bloch did not say at this point what “the soundest comparative method” was—whether it was Type I, Type II, or a third, undefined method. Bloch seemed unwilling to commit himself to Type II because of the apparent complexity of social institutions as compared with the supposed clarity of linguistic affiliations.

In making this distinction between linguistic data as relatively simple and historical data as relatively complex, Bloch said, “Now the historian of societies finds that the facts themselves impose upon him this formidable hypothesis of ‘mixtures.’”<sup>57</sup> Yet, in the *Cambridge Economic History of Europe*, he traced the seignorial system in its various manifestations back to a common origin, the “rustic chieftain.” The rustic chieftain may have been an accurate reconstruction, or perhaps it will turn out to be a “mixture.” In fact, since the late nineteenth century, comparative historical linguists have known that Proto-Indo-European was composed of dialects. In both cases, however, we do not have to resort to the theory of a “mixture”; rather, we may conclude that the search for common origins has not been entirely successful and that more work needs to be done.<sup>58</sup>

<sup>54</sup> Bloch, *Feudal Society*, 446–47 (*La Société féodale*, vol. 2: *Les Classes et le gouvernement des hommes* [Paris, 1949], 250–51).

<sup>55</sup> Bloch, *Feudal Society*, 446 (*La Société féodale*, 250: “La tâche sera facilitée par d’excellentes études, marquées déjà au coin de la plus saine méthode comparative”).

<sup>56</sup> For his endorsement of Type II comparisons in 1939, see Bloch, “Problèmes d’histoire comparée,” *Annales d’histoire sociale*, 1 (1939): 439–40: “La vérité profonde est, d’ailleurs, qu’un tour d’horizon d’une pareille ampleur était, vraisemblablement, beaucoup trop ambitieux. J’ai essayé de le montrer naguère: ce n’est pas vers l’exemple du *Rameau d’Or* que, pour assurer son avenir, l’histoire comparée de la structure sociale doit aujourd’hui se tourner. Ses modèles, il lui faut les demander, beaucoup plutôt, aux méthodes, plus sûres, des linguistes. De même que ceux-ci ont commencé par élaborer, solidement, une linguistique indo-européenne ou intersémitique, par exemple, le premier soin qui s’impose à nous est de bâtir—sans négliger, bien entendu, d’élargir, par instants, notre vision—une histoire comparée des sociétés européennes.”

<sup>57</sup> Bloch, “Towards a Comparative History,” trans. Anderson, 68 (“Pour une histoire comparée,” 42).

<sup>58</sup> Meillet, *The Comparative Method in Historical Linguistics*, chap. 2: “Common Languages,” 25–35. Meillet stated, “For all the groups now established and studied in a proper manner, the way to make a comparison is to posit an initial ‘common language’ (German *Ursprache*)”; *ibid.*, 25. Here again Meillet insisted upon reconstruction as the *sine qua non* for comparison, but he was quick to point out that we are seldom able to compare



Bloch's use of the word "facts" raises a serious objection. To quote one recent scholar on the subject, "Historical facts are . . . not given. They are rather postulates designed to explain the characteristics of contemporary data relating to the past or future."<sup>59</sup> The new methods of historians frequently require a different set of "facts" if they are going to be programmed into the computer; new topics of study or the juxtaposition of old subjects in a different pattern is sometimes mandatory if one is to employ an untried method or pose questions that have hitherto been overlooked.

That Bloch's treatment of the comparative method at the end of *Feudal Society* is inconsistent with his earlier pronouncements is comprehensible given his final definition of feudalism. That definition was obviously not meant to be a product of Type II comparative technique so much as it was an attempt to grasp the *structure* of the society, a goal that need not involve a search for origins. Arguments about methodology, both structural and historical, continue to the present time.<sup>60</sup> Linguists have not yet solved this problem either, although the most persuasive and successful historical linguists were also structuralists—that is, they considered the data from the synchronic and the diachronic perspectives, as did Bloch himself.

SEPARATING EARLY STRUCTURALISM FROM THE WORK that has appeared since Marc Bloch's death in 1944 is necessary at this point. In the preface to Jacques Derrida's famous *Of Grammatology*, Gayatri Spivak wrote, "A structure is a unit composed of a few elements that are invariably found in the same relationship within the 'activity' being described. The unit cannot be broken down into its single elements, for the unity of the structure is defined not so much by the substantive nature of the elements as by their relationship."<sup>61</sup> This definition of structure has an ancient and venerable tradition in the history of philosophy. The point can be demonstrated by placing Aristotle's definition of tragedy side by side with Bloch's definition of feudalism:

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our hypothetical reconstructed forms with "known reality." He then elaborated upon the limitations of the comparative method, limitations that reflect the complexity of linguistic analysis, not its simplicity.

<sup>59</sup> Traian Stoianovich, *French Historical Method: The Annales Paradigm* (Ithaca, N.Y., 1976), 36, 36 n. 25. Also see *ibid.*, 103–04.

<sup>60</sup> Stoianovich, for example, stated, "On the subject of comparative method, however, there is a modicum of disagreement. One of the early leaders of the *Annales* School, Marc Bloch, held that 'there is no true understanding without a certain range of comparison; provided, of course, that comparison is based upon differing and, at the same time, related realities.' On the other hand, the cultural anthropology of Claude Lévi-Strauss, who has disciples in the *Annales* School, teaches that a comparative method is rarely likely to be useful since the data introduced for purposes of comparison may be temporally or spatially so close that one cannot be sure of dealing with separate phenomena, or the allegedly related realities may be so heterogeneous that they in fact involve a comparison of incomparable things." *French Historical Method: The Annales Paradigm*, 37. And Eisenstadt declared, "Some skepticism has recently been voiced about the extent to which such comparison may be of any value beyond the range of societies that are culturally and geographically very similar"; "Social Institutions: Comparative Study," 425.

<sup>61</sup> Spivak, Preface to Derrida, *Of Grammatology*, lv.

A tragedy, then, is the imitation of an action that is serious and also, as having magnitude, complete in itself; in language with pleasurable accessories, each kind brought in separately in the parts of the work; in a dramatic, not in a narrative form; with incidents arousing pity and fear, wherewith to accomplish its catharsis of such emotions.<sup>62</sup>

A subject peasantry; widespread use of the service tenement (i.e. the fief) instead of a salary, which was out of the question; the supremacy of a class of specialized warriors; ties of obedience and protection which bind man to man and, within the warrior class, assume the distinctive form called vassalage; fragmentation of authority—leading inevitably to disorder; and, in the midst of all this, the survival of other forms of association, family and State, of which the latter, during the second feudal age, was to acquire renewed strength—such then seem to be the fundamental features of European feudalism. Like all the phenomena revealed by that science of eternal change which is history, the social structure thus characterized certainly bore the peculiar stamp of an age and an environment.<sup>63</sup>

That there is more agreement among scholars about the term “tragedy” than there is about “feudalism” does not invalidate the suggestion that both of these definitions stress the relationship of the elements composing the “activities.” If tragedy is a well-defined literary genre and feudalism is a vaguer word defined in radically different ways, it is nonetheless true that Bloch’s definition sought to get at the structure of the society that he called “feudal”—that is, to show the relationship of the parts—just as did Aristotle in his description of tragedy.

Definitions of structuralism that apply to the post-World War II generation tend to be synchronic rather than diachronic, by and large. Both Bloch and the postwar structuralists went to the linguists for a methodology that would permit them to explore questions in new and more effective ways. Modern structuralists have kept the objective defined herein—that is, a focus upon relating parts to the whole—but they have refined their definitions. Recently, Michael Lane summarized these refinements:

1. All patterns of human social behaviour are codes, with the characteristics of languages.
2. Man has an innate structuring capacity which determines the limits within which the structure of all types of social phenomena can be formed.
3. Relations can be reduced to binary oppositions . . . in that any universe . . . can be divided into classes of  $\alpha$  and  $\sim\alpha$ .<sup>64</sup>

<sup>62</sup> Aristotle, *Poetics*, trans. Ingram Bywater, in *The Basic Works of Aristotle*, ed. Richard McKeon (New York, 1941), 1460.

<sup>63</sup> Bloch, *Feudal Society*, 446.

<sup>64</sup> Lane, “Introduction,” in Michael Lane, ed., *Introduction to Structuralism* (New York, 1970), 18. Note that Lane, in point 3, stated that “any universe” can be divided into  $\alpha$  and  $\sim\alpha$  (or p and not p). This is certainly true, but it does not prove that binarism lies at the heart of the problem. Although popular in many quarters, binarism as a tool has led scholars into simplistic explanations. Some modern logicians have discarded it as a necessary part of any system. As John E. Pfeiffer wrote, “Modern logic has abandoned one of Aristotle’s most basic principles: the law of the excluded middle, meaning that a statement must be either true or false. In the

These dogmas show the durability of basic structuralist principles; but they also show that structuralists have followed the linguists to the extent that now the data themselves are perceived as having the character of language. On this point, Bloch might well demur, and so might many other scholars in any field of endeavor.

Nevertheless, it seems safe to say that social scientists and others have been impressed with the insights of the linguists, and this phenomenon goes back more than a century. In his seminars, Leopold von Ranke used philological analysis to interpret historical documents, and "comparative philology" was the precursor of "comparative linguistics."<sup>65</sup> Just so, Bloch turned to Meillet. Lévi-Strauss paid homage to Saussure as the linguist who first set forth the principles of modern structuralism and then actually put those principles into operation in anthropology. Linguistics is one of the few humane sciences that have been judged capable of competing with the physical sciences in putting forth hypotheses that have later been confirmed by "facts."

Bloch turned not only to linguistics but also to a variety of other disciplines—sociology, anthropology, archeology, psychology, geography, and medicine—in order to gain a more profound understanding of the problems that engrossed him.<sup>66</sup> As Bryce Lyon has said, "Marc Bloch lived life as he wrote history: he always chose the difficult and spurned the easy. Like some historians, he could have effortlessly described simple events and dodged issues, but such history disgusted him."<sup>67</sup> Hence, Bloch's use of linguistics can be seen as a contribution toward an interdisciplinary approach that, though perplexing, was nonetheless worthwhile as a noble and unfinished experiment.

In short, neither Bloch nor his successors ever really attempted Type II comparison, with the exception of his analysis of seignorial forms, which led him to the "rustic chieftain." Type II was dismissed as being unworkable in history because institutions and societies were considered to be unmanageable units or because the evidence seemed to lead back to "mixtures," a phenomenon that even the linguists said would make the comparative method impossible. Neither Bloch nor his followers, however, tried to define what units in historical data might correspond to the phoneme and the morpheme in language, and without these units linguists could never have embarked upon the study of syntax and

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new system a statement may have three values: true, false, or indeterminate. A close analogy to this system in the legal field is the Scottish trial law, which allows three verdicts—guilty, not guilty, or 'not proven.' " Pfeiffer, "Symbolic Logic," in Morris Kline, ed., *Mathematics in the Modern World: Readings from Scientific American* (San Francisco, 1968), 211.

<sup>65</sup> Theodore H. Von Laue, *Leopold Ranke: The Formative Years* (Princeton, 1950), 14, 15: "Philology and grammar were the established tools by which men could lift the film of unfamiliarity from past ages and distant civilizations. . . . The profit of these studies to Ranke was fourfold: a thorough knowledge of ancient literature and languages, an ultimate acquaintance with ancient historiography, particularly Thucydides, a method of historical criticism, and finally a technique for advanced training, the seminar." Ranke, however, never called his class a "seminar"; rather, he used the Latin word *exercitationes* ("exercises" or "practices"); see Edward Gaylord Bourne, *Essays in Historical Criticism* (New York, 1901), 268.

<sup>66</sup> For an even more open-ended call to interdisciplinary history, using a universal or Type I comparative orientation, see Erich Rothacker, "Die vergleichende Methode in den Geisteswissenschaften," *Zeitschrift für vergleichende Rechtswissenschaft*, 60 (1957): 13–33.

<sup>67</sup> Lyon, Foreword to Bloch, *French Rural History*, x.

semantics with such an apparent degree of success.<sup>68</sup> In effect, historians have made comparisons at the syntactic and semantic levels without first establishing the components of these larger structures.<sup>69</sup>

William Sewell thought that Bloch's "rules about the units of comparison" needed elaboration: "First, *the units to be used in an inquiry will vary not only with the aspect of social life being studied and with the 'historical instant,' but also with the particular explanatory hypothesis we are trying to test by our comparison. . . . And, second, the units to be used for comparison need not be geographical units.*"<sup>70</sup> By contrast, linguists are at a distinct advantage, for their units—the phoneme, the morpheme, the taxeme (a unit of syntax), and the sememe (a unit of semantics)—are applicable to all languages of the world, whether they are genetically related or not, and at any "historical instant."

Until systematic attempts to formulate such units are made, it will be impossible to say whether or not historians can use Type II comparison. There may well be a variety of ways to approach the comparison of societies that do not fall into Type II or even into Type I, and the results may be enlightening and useful to the reader. It would, however, be interesting, after a half century of discussion about the comparative method, if Type II were actually put into practice and either proved or disproved as a valid tool for the working historian.

<sup>68</sup> Bloomfield defined the phoneme as "a minimum unit of distinctive sound-feature"; *Language*, 79 (Bloomfield's italics). And he defined the morpheme as "a linguistic form which bears no partial phonetic-semantic resemblance to any other form [and] is a simple form"; *ibid.*, 161 (Bloomfield's italics). For a full explanation of these rather cryptic definitions, see *ibid.*, chap. 5: "The Phoneme," 74–92, chap. 6: "Types of Phonemes," 93–108, chap. 13: "Morphology," 207–26, chap. 14: "Morphologic Types," 227–46.

<sup>69</sup> The most significant advances in nineteenth-century linguistics came first in phonology and then in morphology, areas stressed by Bloomfield, whose book *Language* was first published in 1933. Although syntax and semantics were known in the nineteenth century, advanced study of these two areas became more prevalent in the twentieth century, especially after World War II. Noam Chomsky's *Syntactic Structures*, published in 1957, started a new wave of interest in syntax, as did his other works, including *Aspects of the Theory of Syntax* (Cambridge, Mass., 1965), and "Explanatory Models in Linguistics," in E. Nagel *et al.*, eds., *Logic, Methodology, and Philosophy of Science: Proceedings of the 1960 International Congress* (Stanford, 1962), 528–50. For an analysis of Chomsky's work, see John Lyons, *Noam Chomsky* (New York, 1970). Also see Emmon Bach, *An Introduction to Transformational Grammars* (New York, 1964); and Andreas Koutsoudas, *Writing Transformational Grammars: An Introduction* (New York, 1966). For recent developments in semantics as well as in the more comprehensive and rapidly developing field of semiotics, see John Lyons, *Semantics*, 2 vols. (New York, 1977); Roman Jakobson, *Coup d'oeil sur le développement de la sémiotique* (Bloomington, Ind., 1975); Umberto Eco, *A Theory of Semiotics* (Bloomington, Ind., 1976); Michel Galmiche, *La Sémantique générative* (Paris, 1975); Terence Hawkes, *Structuralism and Semiotics* (Berkeley and Los Angeles, 1977); and, above all, the various works of Thomas A. Sebeok. In addition to editing, contributing to, and writing many books in the field, Sebeok is also the editor-in-chief of *Semiotica: Journal of the International Association for Semiotic Studies* (The Hague, Paris, and New York, 1969–) and the executive director of the Semiotic Society of America, which publishes a bulletin called *Semiotic Scene*.

<sup>70</sup> Sewell, "Marc Bloch and the Logic of Comparative History," 213 (Sewell's italics).

## Comments:

LINGUISTICS, as Alette O. and Boyd H. Hill point out, has long had a reputation as the most rigorous of the human sciences. This reputation has often attracted anthropologists, historians, and other social scientists to linguistics as a model for their own research—a more appropriate one, perhaps, than the model of Newtonian physics generally preferred by economists and quantitative sociologists. The Hills' essay is an account of one attempt—not in their eyes a very successful one—to model historical investigation on linguistics. Their exposition of Antoine Meillet's historical linguistics is clear and informative, and their demonstration that Marc Bloch's comparative history did not conform to the linguists' rules is entirely convincing. Bloch's analogy between comparative history and historical linguistics was, to put it bluntly, misconceived and misleading.

What conclusion should we draw from this fact? Here I part company with Alette and Boyd Hill. They think that Bloch's mistaken analogy was a fatal flaw, that Bloch's own writings on comparative history "lack precision and coherence" (page 837), and that a strict application of the methods of historical linguistics is the only way to make comparative history more precise. The method they advocate has two essential features. First, the goal of comparative history should be the reconstruction *in a prehistorical past* of the remote common origin of later-divergent but genetically related social structures—a sort of *Urvivilisation* that would correspond to the linguists' *Ursprache*. Second, historians should define "units of historical data" that "correspond to the phoneme and the morpheme in language" (page 845). Without such elementary units ("institemes"? "technologemes"?), the study of more complex social structures—the historical equivalents of syntax and semantics—is doomed to failure.

AS A PROGRAM FOR COMPARATIVE HISTORY, this position is unacceptable. The project of defining units of historical data that correspond to phonemes and morphemes hardly seems promising, especially since Alette and Boyd Hill fail to provide any clues as to what such units might be. And to maintain that comparative history should be confined to the reconstruction of common origins, while more plausible on the surface, is equally unacceptable.<sup>1</sup> In the first place,

<sup>1</sup> One of the strictures the Hills offer as a part of their reconstructive program is quite valuable, although perhaps not for the reasons the Hills think. This is their "all and only" rule—that historians must examine "*all and only* the phenomena in a related group" (page 834). Although I do not think it is possible to delineate "a related group" of the sort they envisage (a group of societies descended from a remote common origin), the Hills are right to remind us that comparisons of just two societies can often be misleading. More often than not, the proper comparative universe contains appreciably more than two cases. Bloch was certainly aware that comparisons should embrace an entire class of phenomena. In 1928, for example, he pointed out at some length that no attempt to explain the rise of representative institutions in the fourteenth and fifteenth cen-



it would mean returning to an obsessive concern with origins, a concern that Bloch criticized with particular effectiveness. But the Hills' strategy goes beyond a general fascination for origins. They want historians to focus their attention on prehistoric origins, to construct by means of controlled speculations a past society about which we have no direct evidence. That this strategy has been fruitful in historical linguistics is undeniable. But to advocate such a program for comparative history is to ignore fundamental differences between languages and societies. The Hills' program not only would make comparative history irrelevant to all of the major issues of current historical research but also would commit us to a strategy that cannot possibly succeed even on its own chosen terrain of reconstruction.

A strict application of linguistic methods to comparative history is impossible because social structures differ in character from linguistic structures; it simply is not true that "all institutions behave as languages do" (page 831). The essential difference lies not in their complexity<sup>2</sup> but in the arbitrary character of the linguistic sign. That is to say, there is no intrinsic connection between the sign (for example, the word "chicken") and the thing signified (a particular type of bird); the same thing could just as well be signified by another sign ("sparrow" or "floor" or "instead" or "Hauptpunkt" or any other combination of sounds that the human ear can distinguish). It is this arbitrariness that makes linguistic structures so stable. How could the Indo-European language have survived the epochal change from nomadism in Central Asia to settled agriculture and the rise of cities in Central or Southern Europe without undergoing changes more fundamental than the loss of an intervocalic /s/ or the "First Sound Shift"? How can inhabitants of societies as different in their fundamental constitution as Hindu India, Ancient Greece, and present-day California or Scandinavia express their thoughts in languages with the same essential structures? Precisely because the system of sounds and the rules for combining them into comprehensible statements which we call the Indo-European language bear no intrinsic relationship to the world inhabited by either the Indo-Europeans or their scattered descendants.

When we turn to social structures, this crucial feature of arbitrariness is absent. While we can analytically separate the rules of social structure from the activities to which they apply, the relationship between rules and activities is any-

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turies can hope to succeed without taking into account *all* of the cases: the French *États*, the German *Stände*, the Spanish *Cortes*, the Italian *Parliamenti*, and the English Parliament; Bloch, "A Contribution Towards a Comparative History of European Societies," trans. J. E. Anderson, in Bloch, *Land and Work in Medieval Europe: Selected Essays of Marc Bloch* (New York, 1969), 55-56.

<sup>2</sup> The Hills go to great lengths to refute what they see as Bloch's insinuation that linguistic problems are less complex than historical problems. But Bloch claimed nothing of the kind. He did say, when speaking of the linguists' success in reconstructing the Indo-European language, that "the history of social organisation is in this respect a much more difficult problem. The fact is that a language presents a much more unified and easily definable framework than any system of institutions: hence the relative simplicity of the problem of linguistic affiliations." Bloch, "Towards a Comparative History," 68. In this passage Bloch is not claiming that linguistic structures are simpler than institutional structures in some global sense, only that they are "more unified and definable" and therefore that *affiliations* are easier to trace. Anyone charged with the task of drawing up a historian's equivalent of "Grassmann's Law" would surely agree.

thing but arbitrary. Take, for example, the rules of the *seigneurie*. These could only apply to a system of settled agriculture; in a nomadic or hunting and gathering society, they would be nonsense. As Bloch and others have demonstrated, moreover, the rules of *seigneuries* are dependent upon (and therefore change with) technologies, man-land ratios, the availability of money, religious views about slavery, and so on. In all of these and many other ways, the rules of *seigneuries* have definite and nonarbitrary relations to a world outside themselves. At the same time, the social existence to which seignorial regulations apply is constituted by those regulations in a way that a chicken is not constituted by the syllables that form the word or by the linguistic rules that enable us to define a chicken as "a common domestic fowl." The structure of the rules of the *seigneurie* and the social activities to which they apply are bound by a whole complex of mutual determinations.

Epochal changes in social life leave linguistic structures virtually unchanged, while such changes as migration from Central Asia to Greece or the Gangetic plain or the change from nomadism to settled agriculture obliterate ancient institutional structures or transform them beyond recognition. To postulate that such changes could be described by historical equivalents of Grimm's Law or Grassmann's Corollary is to ignore the difference between language and society—a difference so fundamental as to make the linguists' method of reconstruction utterly illusory as a program for comparative history. Thus, while it is fair to criticize Bloch for loose thinking when he likened his style of comparative history to historical linguistics, it is misguided to criticize him for not taking his own loose and inaccurate analogy more literally.

Alette and Boyd Hill, of course, claim that Bloch actually carried out their program of reconstruction in his essay on the rise of seignorial institutions, tracing the origins of the *seigneur* back to the primitive "rustic chieftain." But, on closer inspection, this claim turns out to be false. Bloch did not treat "rustic chieftainship" as a *common* origin of *seigneuries* in the sense that Indo-European was the common origin of Sanskrit, Greek, Latin, and the Germanic and Slavic tongues—that is, a single unified structure that slowly evolved into a divergent family of structures with precisely identifiable relations to the *Ur*-structure. By introducing "rustic chieftains," Bloch was merely pointing out that some form of local potentates existed in European villages long before the emergence of the *seigneurie* proper. Far from regarding them as a single common origin of the *seigneurie*, Bloch remarked that "the more we knew about [these village chiefdoms], the more varied we should probably find them."<sup>3</sup> The truth is that Bloch himself considered the linguists' strategy of reconstruction to be improbable as a guide for historical research. "Comparative history," he remarked ironically, "may reveal hitherto unnoticed interconnections between human societies, but it would be excessive optimism to expect that it would lead to the discovery of

<sup>3</sup> Bloch, "The Rise of Dependent Cultivation and Seignorial Institutions," in J. H. Clapham and Eileen Power, eds., *The Cambridge Economic History of Europe*, vol. 1: *The Agrarian Life of the Middle Ages* (2d ed., Cambridge, 1966), 286.

fragments of an ancient and hitherto unknown parent society.”<sup>4</sup> While Bloch was sometimes imprecise in his theoretical statements, he also knew when to step back from misleading abstractions or forced analogies.

THE OBTVERSE OF ALETTE AND BOYD HILL’S CONTENTION that comparative history must follow the rules of historical linguistics in order to be coherent is their contention that Bloch’s comparative history, because it failed to follow these rules, was incoherent. Once again, I disagree. The point of my own article, which the Hills cite, is that, for all of Bloch’s lack of rigor as a theorist, he elaborated *in practice* a comparative method that was consistent, coherent, and appropriate to the subject-matter of history.<sup>5</sup> All of Bloch’s comparative history, as I argued there, was based on a unified logic—a modified experimental logic of hypothesis testing. Because of Bloch’s underlying consistency, his practice of comparison can be used to elaborate a set of methodological rules that differ essentially from the rules of historical linguistics and that Bloch’s own theoretical writings express only partially and imperfectly. The rules implicit in Bloch’s historical practice constitute a valid methodology for comparative history, one that, in my opinion, is still insufficiently understood. My article, by working out and stating these implicit rules, was an attempt both to demonstrate the coherence of a great master’s historiographic practice and to make his method more accessible to the community of historians.

It is true that Bloch’s analogy between comparative history and comparative linguistics is misconceived. But, by fixing on surface inconsistencies in Bloch’s argument and by failing to examine seriously his practice, Alette and Boyd Hill have failed to discover the deep consistencies that give Bloch’s work its continuing power and interest.

WILLIAM H. SEWELL, JR.  
*University of Arizona*

<sup>4</sup> Bloch, “Toward a Comparative History of European Societies,” trans. Jelle C. Riemersma, in Frederic C. Lane and Jelle C. Riemersma, eds., *Enterprise and Secular Change: Readings in Economic History* (Homewood, Ill., 1953), 516. The Anderson translation of this passage in *Land and Work in Medieval Europe* is hopelessly garbled.

<sup>5</sup> William H. Sewell, Jr., “Marc Bloch and Comparative History,” *History and Theory*, 6 (1967): 208–18.



TWENTIETH-CENTURY ACTIVITY in comparative study of the past is a potentially important theme in intellectual history. There has been able writing on it from anthropologists, but much more is needed from the various points of view of other specialists. Right now there is room for what Marc Bloch called premature syntheses, designed to map out the main problems of a subject without

burdening the reader with microscopic detail. In these days of fast food service there is demand for quickie versions of that genre in the form of journal articles. Some of the supply is from people who are kept too busy teaching to give their ideas a workout in new research but who want a chance to get them into circulation. In "Marc Bloch and Comparative History," the *American Historical Review* got both less and more than its editor bargained for. It is not a "think piece"; it is a piece of genuine myth-making—unpolished but, unconsciously, in the grandest tradition. Marc Bloch, comparative methods, and the wisdom of great linguists—all are symbols. As scholarship, the essay is careless and superficial; as myth, these faults are essential, uncorrectable, and fascinating.

Consider what Alette O. and Boyd H. Hill call the "logic" of their argument. It is a sequence of five positions, as follows: (1) Bloch, seeking wisdom, derives his conception of comparative methods from the greatest French linguist of his time; (2) through some flaw in his mind or character he fumbles, he misapplies them; (3) he becomes the "father" of a flawed bag of tricks labeled "comparative history"; (4) this flawed thing is after his death perpetuated and spread by "disciples"; (5) wisdom grows among the linguists, and anthropologists pick up some of it; but historians dither, or they sit blindly in the shadow of the authors' necessarily imperfect mythic hero.

Now, anyone familiar with Bloch's writings and with the intellectual temper of the historians' world that he graced can tread all of this supposed reasoning into the ground. Indeed, it is enough to knock out the first position, by evidence long in print. The rest of the structure then collapses of its own accord, but each part of it, if one has patience, can be reduced to dust by the weight of indisputable evidence. I spent a lot of time trying to draft a comment advising the Hills to look at one or another of their scraps of alleged evidence with more sensitivity to context and so forth. This became a dreary and depressing chore. Only when a metaphor came to my mind, suggesting that their picture of Bloch has some affinities with the figure of Moses muddleheadedly losing his way in the desert and failing in his divinely ordained mission, did I begin to see that perhaps I was missing the whole point of the paper. Impatient with its dodging of all of the larger issues that surround the development of comparative methods and with its cavalier handling of the meager sources cited, I had failed to realize that what is important to me is, to the Hills, irrelevant. From the mythic standpoint the five positions are indissolubly linked by the ahistorical errors they embody; these are like articles of faith.

Their first position, for example, assumes not only that Bloch received his conception of comparative methods (including his distinction between procedures applicable to comparison of analogous phenomena in societies that were unrelated and those appropriate to comparison of similar phenomena in the closely related societies of the western half of Europe) from the book that Antoine Meillet published on comparative method in linguistics in 1925 but also that Bloch subsequently fell so deeply under the spell of linguistics as to be planning, by 1928, to switch his research from France to Eastern and Central European language communities. Here the Hills suppose that he would somehow be

transposing Meillet's "techniques" from the study of language families to the study of societies. A reader unfamiliar with Bloch's work would not be able to spot its distortion in this account, but the progress of his thinking on comparison is readily traceable in the three monographs that Bloch had published in 1913, 1920, and 1924. In the last of these, his book on the history of popular belief in royal healing powers in Western Europe, the need to refute an engaging hypothesis of Sir James Frazer (entailing comparison between Polynesian chieftains as described by missionaries and early medieval English kings) led him to make a lucid methodological statement in terms of concepts of explanation that would be satisfactory to historians.<sup>1</sup> The Hills had not surprisingly overlooked both this and Bloch's explicit statement in his address at Oslo to which they refer, after praising Meillet's stand, that he would be speaking from the historian's point of view. Their idea about his supposed intention to switch his research to Eastern and Central Europe under Meillet's guidance also involves them, as anyone who checks the French text of the Oslo address may verify, in a double-headed monster howler in misreading a simple sentence and by following a careless translator into taking *occidentale* to mean "eastern."<sup>2</sup> Perhaps in the mythic world the sun sets in the east. More pertinent to nonmythic interests is what the misreading of that passage reveals: the Hills have entirely overlooked the four following sections of the Oslo address, in which Bloch illustrates the uses of comparison in European history.

The second position is set up by giving the two types of comparison that Bloch outlined at Oslo labels and criteria appropriate not to his own exposition of historical comparison but to Meillet's discussion of linguistic comparison. The Hills then berate Bloch for not following their own interpretation of what the latter demanded. They fall into numerous confusions that cannot be untangled in my allotted space. The most misleading, to nonmythic minds not knowing the text or the 1924 statement, is the idea that he "rejected" the broader, more general type of comparison. European medievalists did not in any case practice it, but he discerned four advantages to it, two of which were similar to those of more careful, closer comparisons. Procedures differed, but the results were not entirely different in kind. On the question of his supposed confusion regarding the problem of "origins," the Hills have a real romp, conflating three quite different kinds of inquiry.

The mythic Bloch, to fulfill his symbolic role, had to "father" something, and the child had to be unsatisfactory.<sup>3</sup> Personally, I think Bloch was mistaken in hanging on to the term "comparative history," which is just about as silly as the term "comparative religion." Such terms had become common while he was a boy, and they inevitably became more and more ambiguous with time. It is inaccurate even to call him an adoptive father. He ignored the historical com-

<sup>1</sup> Bloch, *Les Rois thaumaturges: Étude sur le caractère surnaturel attribué à la puissance royale particulièrement en France et en Angleterre* (reprint ed., Paris, 1961), 51–59.

<sup>2</sup> See the Hills' seventh paragraph, page 831, above.

<sup>3</sup> To the nonmythic reader, I commend Philippe Wolff, *Western Languages, 500–1500 A.D.* (New York, 1973), which demonstrates the continuance and deepening of what Marc Bloch regarded as "a happy alliance" between linguists and historians.



parisons of German sociologists, and it is impossible to judge the extent of his influence. The two great weaknesses of the third and fourth positions, to the nonmythic mind, are that many people, as I recollect clearly from my own contacts over the years, shared some of his views on method without even knowing his work and that comparative methods came to be more and more diversified, in all disciplines, with changes in the character of the kinds of enquiry they served.

This last observation may help explain why the Hills necessarily, with the aid of their usual misreading of sentences, have to berate me a bit. With all of its elastic imagination, the mythic mind on some points prefers inexorable rigidity. The chief purpose of the international quarterly for which I was initially and for over fifteen years responsible was to keep discussion going about how comparative methods, whenever and wherever they got into a rut, could be shaken into more fruitful lines. The Hills' contentment with the two-type classification as they interpret it is from that point of view oddly naive. I do, however, plead slightly guilty regarding what could be a slightly rational cause of their finding only one article, apparently, in the several hundred for which I was responsible, editorially speaking, that they liked. If I had obtained more from linguists they might have liked me better. The reason I did not was because those years saw very intensive internal debate between different branches of their discipline. The quarterly served those who needed it most.

I am writing more elsewhere on this whole subject and, meanwhile, wish Alette O. and Boyd H. Hill luck.

SYLVIA L. THRUPP  
*University of Michigan*

## Reply:

WE ARE GRATEFUL FOR THE COMMENTS on "Marc Bloch and Comparative History" because they confirm our suspicion that comparative history as a field is in profound need of definition. For the careful reader it should be apparent that we do not insist upon a "program" for all historians to follow. Indeed, we make it quite clear that Type II comparison is only one of many possible approaches to the study of societies. We merely point out that Bloch never constructed a Type II model for historians—and neither has anyone else. Furthermore, we do not agree with William H. Sewell, Jr., in claiming that Bloch's analogy was "misconceived and misleading" (page 847). "Misleading," yes, but "misconceived," no. Bloch's suggestion about the possibility of using the methods of the comparative linguists was most exciting. That he did not carry through on it in no way negates its boldness and is no reason to discourage historians from attempting to do so now. If they do not wish to, that is their choice, but anthropologists have investigated the techniques of linguists and found them to be useful.

One prominent linguist has summarized the way in which anthropologists have employed linguistic methods and models:

Linguistic methods and models can neither be rejected out of hand, [n]or be made to apply by sleight of hand. As to the latter, attempts at liberal transplantation have usually failed, through being based on surface analogy alone. Where fundamental principles have been understood, there has been some success. The ethnographic work of Goodenough, Conklin, C. O. Frake, and others, for example, has seen its task as one of understanding the nature of culture and of cultural description from the same vantage point as that from which modern linguistics understands the nature of language and linguistic description. Rather than seek "vowel triangles," "phonemes," or "deep structures" of culture, *it has sought to understand and apply the methodological principles which lead to the discovery of phonemic and other structures—principles of contrastive relevance, distributional class and paradigmatic set, distinctive feature, transformational relation—and to discover their sphere of applicability in other cultural phenomena.*<sup>1</sup>

Dell Hymes's statement shows that, rather than transferring wholesale a set of terms from one field to another, the nonlinguist must apply the principles that underly linguistic structures in order to invent a set of terms that will be useful to other disciplines, in this case history.

THE PRINCIPAL SOURCE of Professor Sewell's criticism lies in a commonly held assumption that history, unique in its subject-matter, is in no way analogous to

<sup>1</sup> Hymes, "Anthropology and Sociology: An Overview," in Thomas A. Sebeok, ed., *Current Trends in Linguistics*, vol. 12: *Linguistics and Adjacent Arts and Sciences* (The Hague, 1974), 1458 (italics added).

language. Many other historians, making similar assumptions, appear to dismiss Bloch's statements on methods when it suits them while drawing upon his inconsistencies as a kind of *carte blanche* to pursue any sort of comparison under the sun. We take Bloch at his word, both in his statements on method and in his history, rather than deriving a "method" that others claim can only be mystically gleaned from his history. If there is a "method" in Bloch's writings, why has it not been set forth coherently by his interpreters? Sewell's characterization of him as a "hypothesis tester" is scarcely precise enough to be helpful. Why has he dogmatically asserted, once again, that historians cannot make use of linguistics when other social scientists have moved into the area and applied its principles to their own advantage? We see no reason to warn scholars against trying Type II comparisons as if such an experiment posed a threat to all other comparative historians.

Professor Sewell further ridicules our article for its "obsession" with "origins" and, thereby, dismisses us as prehistorians (page 848). His remarks are, to say the least, seventy-five years out of date. We agree with the position taken in 1904 by Carl Darling Buck, an Indo-European linguist of the first rank:

I have said that the comparison of related forms was not an end in itself, but a means of reconstructing the parent form. But I do not wish to imply that these parent forms are of great intrinsic interest or that the reconstruction of the parent speech is the ultimate aim. No one is ambitious to speak this hypothetical language, nor does it . . . furnish the key to the problems of primitive linguistic development. Indeed, this language which we arrive at by reconstruction is itself a highly developed form of speech, which has behind it thousands of years of history which is forever inaccessible to us.

Its value lies rather in the light which is thereby reflected on the history of each individual language belonging to the group. Each language contributes its share of evidence for the reconstruction of the parent speech, and each in turn is illuminated by it. The real object throughout is to trace the development of a linguistic phenomenon from its earliest attainable stage to its latest expression. Comparative grammar is simply a history of a group of related languages, and when that is said, its relation to the history of an individual language of the group is obvious. They are not different sciences, one merely auxiliary to the other, but represent a wider and a narrower range of the same subject. Whatever differentiation exists is consequent only upon a division of labor. The historian of the Greek language, for example, is, from the purely linguistic standpoint, a specialist within the Indo-European field. And if the wider outlook of comparative grammar is essential to the intelligent study of the history of the individual language, it is no less true that comparative grammar depends for its very existence upon the investigation of the special facts and conditions of each language.<sup>2</sup>

So much for our "fascination" with an "Urzivilisation."

As to Professor Sewell's criticism of the "arbitrariness of the sign" (page 848), a feature of Antoine Meillet's method that Bloch did not include in his 1928 article, our point was to show that this omission revealed Bloch's unwillingness or inability to comprehend the full message of the comparative linguists. We do not have space here to explore this area of theoretical concern, but we urge the

<sup>2</sup> Buck, "The Relations of Comparative Grammar to Other Branches of Learning," in Howard J. Rogers, ed., *Congress of Arts and Science, Universal Exposition, St. Louis, 1904*, vol. 3: *History of Language, History of Literature, History of Art* (Boston, 1906), 35-36.

reader not to take Sewell's discussion of the "arbitrariness of the sign" as the last word on the subject. This concept is under study not only by linguists but also (and of more relevance to historians, perhaps) by semioticians.<sup>3</sup> A sign need not be a word: a sign can be anything that signifies something else. "Not only are words signs, but also gestures, images, nonlinguistic sounds, like the chimes of Big Ben. Obviously devices (such as flags) created by man in order to indicate something are signs, but so are, in ordinary language, the thread of smoke that reveals a fire, the footprint in the sand that tells Robinson Crusoe a man has passed along the beach, the clue that permits Sherlock Holmes to find a murderer."<sup>4</sup> Thus, regalia, media of exchange, architecture, kinship relations, dress codes, and rituals can be highly arbitrary and also obviously useful to historians. Very few historians have made use of semiotics any more than they have made use of the Type II methods of the linguists, but we believe that such experiments can only enrich the historical profession.

It is flattering to find that Professor Sewell viewed our article as such a menace that he needed to reassure all comparative historians that our so-called program cannot possibly work and should be avoided at all costs. What is disconcerting is that he so often takes our statements out of context and generally twists our language to his own purpose. For example, we emphatically did not state that "comparative history should be confined to the reconstruction of common origins" (page 847). Nor did we say that "Bloch's comparative history, because it failed to follow these rules, was incoherent" (page 850). Our position from the outset has been that Bloch did not follow his own stated plan of 1928, but we nowhere said that Bloch's practice of history was thereby ruined. We believe that Bloch's historical works need no defense, nor do we need to defend our remarks about the confusion engendered by his statements on methods.

As for Sylvia L. Thrupp's bizarre comment, originally entitled "Myth-Making in the Mountains,"<sup>5</sup> only one point deserves a reply: she is quite correct that "occidentale" means "western." Accordingly, we should have constructed our hypothetical analogy around Spain and France, using the Basque-speaking area as the non-Indo-European equivalent to the Hungarians as against the Germans and Slavs. The result of our inquiry would, nevertheless, have been the same.

<sup>3</sup> See Roman Jakobson, *Coup d'oeil sur le développement de la sémiotique* (Bloomington, Ind., 1975); and Umberto Eco, *A Theory of Semiotics* (Bloomington, Ind., 1976). Saussure's "l'arbitraire du signe" was modified by Émile Benveniste and was then redefined by Alfons Nehring; see Benveniste, "Nature du signe linguistique," *Acta Linguistica: Revue internationale de linguistique structurale*, 1 (1939): 23-29; and Nehring, "The Problem of the Linguistic Sign," *Acta Linguistica*, 6 (1950): 1-16. Sewell's discussion of the word "chicken" does not begin to come to grips with the subtleties of the problem, which has not yet been satisfactorily resolved in all of its complexity. Semioticians have enriched the debate about "l'arbitraire du signe" by introducing the tripartite scheme of Charles Sanders Peirce (icon, index, symbol) into the study of sign systems. For a brief description of the work of Saussure and Peirce, see Eco, *A Theory of Semiotics*, 14-16. It is immediately apparent by comparing Eco's analysis with that of Sewell that Sewell has reduced "arbitrariness of the sign" to a simple relation between "chicken" and "common domestic fowl" whereas Eco has emphasized the "image" or concept that a word or other sign evokes in the mind of the interpreter. This observation alone should be proof to those conversant with the literature on "l'arbitraire du signe" that Sewell's discussion is simplistic at best.

<sup>4</sup> Umberto Eco, "Looking for a Logic of Culture," in Thomas A. Sebeok, ed., *The Tell-Tale Sign: A Survey of Semiotics* (Lisse, 1975), 11.

<sup>5</sup> *Editor's Note*: The format of the *AHR Forum* does not include titles either to the "Comments" or to the "Reply."

The rest of Thrupp's comment caricatures the issues in our article. We are, apparently, hillbillies who have no business questioning her editorial preface to *Comparative Studies in Society and History* or attempting an analysis of Marc Bloch, our "Moses in the desert." If we are the McDonald's of the Rockies, it should have been easy for Professor Thrupp to outclass us with haute cuisine from La Lutèce, but her comment gives us nothing to feast upon. It is her response, not our article, that amounts to "myth-making" and metaphor. We did not deal in symbol but in textual data, yet she loftily dismisses the substance of what we revealed about Bloch and the tradition of comparative history since 1928.

IT IS APPARENT IN THE COMMENTS of Professors Sewell and Thrupp that they are mainly concerned with preserving the reputation of their own past work, which they perceive as implicitly undermined by our article. Interestingly enough, they now turn against Bloch himself: Sewell says that Bloch's analogy between comparative history and comparative linguistics was "misconceived and misleading," something he did not say in "Marc Bloch and the Logic of Comparative History," which begins with an encomium to Bloch's 1928 article; and Thrupp chides Bloch for failing to incorporate the works of German sociologists in his writings and for perpetuating the term "comparative history," which she considers "silly" (page 852). Bloch seems overnight to have become a scapegoat, since he can no longer be revered as St. Mark. We do not think the idol has feet of clay, but then we never idolized him in the first place. The reaction of Sewell and Thrupp is symptomatic of a lingering malaise in the methods of comparative history, a field we do not consider "silly" but merely in need of the healing touch of some dedicated scholars who are more interested in the real Bloch than in the defense of their own earlier vision of him.

According to Professor Sewell, "The Hills' program not only would make comparative history irrelevant to all of the major issues of current historical research but also would commit us to a strategy that cannot possibly succeed even on its own chosen terrain of reconstruction" (page 848). We do not wish to throw out all forms of comparative history that do not conform to Type II methods. On the contrary, we would like to see Type II attempted and let all scholars judge for themselves whether or not the results are worthwhile. Certainly, this exercise will not destroy other types of comparative history, nor will such an experiment necessarily be "irrelevant to all of the major issues of current historical research."

ALETTE OLIN HILL  
BOYD H. HILL, JR.  
*University of Colorado*



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## Reviews of Books

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### GENERAL

FRANK E. MANUEL and FRITZIE P. MANUEL. *Utopian Thought in the Western World*. Cambridge: Belknap Press of Harvard University Press. 1979. Pp. vi, 896. \$25.00.

For more than a quarter century Frank E. Manuel and Fritzie P. Manuel, husband and wife, have been colleagues in a great enterprise—the study of utopian thought in the Western world. This tremendous book is the sum of their prodigious labors.

They begin with the Judeo-Christian myth of paradise, pass rapidly over ideal visions of society held in the classical and the medieval eras, and then begin a more leisurely march with a long consideration of Thomas More, whose *Utopia* of 1516 coined the word for the genre. They come at last to the present with its frenetic quests for utopia that include not only the many varieties of Marxism but also the cult of the Reverend James Jones that ended in mass suicide on November 18, 1978, in Guyana.

What generalizations can we make from their crowded account of the multitudinous visions? Not many. Utopias are efforts to create worlds where the woes of the authors' times are borne away. (In modern "dystopias," which the Manuels touch on, the woes triumph!) But beyond a love of education, simplicity, and community, few utopians have agreed with each other about the shape of their dreams.

Utopias seem to fall into three classes with a lot of blurring at the edges. Some, like Thomas More's, were never intended to be realized. They are mythic inventions meant to jolt contemporaries into thinking that *something* should be done. Bacon's *New Atlantis* was this sort, and so are many modern tales of science fiction. The writers supposed that some things in their ideal societies might be imitated; More certainly believed in 1516 that England could treat thieves more kindly. But they did not expect their whole vision to come to life on earth. More

said that most things in his *Utopia* were to be wished for rather than awaited.

More common have been the utopias of zealots and planners, men as diverse as Thomas Münzer and Jean Meslier, Saint Juste and Robert Owen, and, though the Manuels do not mention him, Brigham Young. These men tried to wrench the ideal to earth in the here and now and believed that its power—divine or rational and perhaps violent—would flatten the old and raise a new world.

Still others have believed that society was moving grandly on its way to an end as irresistible as it was inevitable. Here are Condorcet and Karl Marx, the social Darwinists, and the Freudian-Marxists like Herbert Marcuse of our own times.

Their legions march through this gargantuan book, and they illustrate a great paradox of our civilization. We have never quite been satisfied with what we are, and the myth of paradise haunts us and woos us to change. But as often as not, the striving for paradise has created hell on earth. The honest yearnings of one age are seen, when brought to pass in another, to be disasters for the human spirit. Thomas More's *Utopia* with its rigid, communal conformity might seem like heaven to the chaotic sixteenth century. But to those who have seen the new orders of our century with their soul-destroying monotony, More's vision is less enthralling. He made no room for artists. The Manuels offer proof enough that, in utopias, planners are kings and artists, if they exist at all, are servants or worse.

In this book the Manuels have themselves built a city of the mind. Their scope is vast, their lines are sure, their vistas serene. Any specialist may see a crack here and there. But no one in our lifetime will better perform their task.

RICHARD MARIUS  
*Harvard University*

ULRICH MICHAEL KREMER. *Die Reformation als Problem der amerikanischen Historiographie*. (Veröffentlichungen des Instituts für Europäische Geschichte Mainz,

number 92.) Wiesbaden: Franz Steiner Verlag. 1978. Pp. viii, 265. DM 64.

The author of this Berlin dissertation describes his intention as an effort "to trace [in nineteenth- and twentieth-century American Reformation historians] the removal of utopian social goals from the spirit of the Reformation and to that extent the Europeanization of American Reformation historiography." Ulrich Michael Kremer treats historians who have in his opinion played a role in this latter process, classifying them as following: the founders (J. Priestley, P. Schaff, A. C. McGiffert, W. Walker); social historians (P. Smith, E. Erikson, H. Grimm, E. Schwiebert, J. Schapiro, R. Cole); Catholic historians (P. O'Hare, C. Hayes, J. P. Dolan, G. H. Tavard, H. McSorley, J. McCue); Reformed historians (G. P. Fisher, W. A. Mueller, J. T. McNeill, R. Kingdon, R. Bainton, G. H. Williams); and Lutheran historians (C. P. Krauth, R. H. Fife, G. W. Forell, L. W. Spitz, J. Pelikan, W. Pauck).

Despite the book's title, this study is less a treatment of Reformation historiography than an essay on American religious consciousness as reflected in the above scholars. The author presents Americans as having been dominated for centuries by "this-worldly optimism about progress" and concern for "universal freedom and tolerance"—a secularization in his view of the original "utopian" vision of the Puritans. He finds these typically American values constantly projected back onto the Reformation and believes that it was only under the sobering crisis of the Depression that American utopianism buckled and the theological ideas of neo-orthodoxy became attractive to the American mind. This also made possible a more profound understanding of the Reformation. According to the author, the essential agents in this latter process were American Lutherans and Catholics, who had long drunk from the sobering wells of European theological scholarship. American Lutherans are especially praised by the author for their role in helping the nation overcome false optimism, anti-intellectualism, and lack of historical consciousness and for making ecumenism possible in sectarian America.

Kremer has presented a vision of American Reformation scholarship that is very flattering to the theologians and church historians of the old country. He has, however, also limited the usefulness of his book by dealing almost exclusively with theologians and church historians and by ignoring so much American Reformation historiography of the last twenty years. Had he pursued his subject on a broader basis and into the 1970s, he would have found American Reformation scholarship far more varied in method, international in bibliography, ambitious in scope, and lucid in style and presenta-

tion than its German counterpart. He would also have discovered that the "Europeanization" of American Reformation scholarship has been a very mixed blessing.

STEVEN OZMENT  
*Harvard University*

VACLAV MUDROCH. *The Wyclif Tradition*. Edited by ALBERT COMPTON REEVES. Athens, Ohio: Ohio University Press. 1979. Pp. xvii, 91. \$8.00.

The subject of this short study is the attention writers of the sixteenth, seventeenth, and eighteenth centuries gave to John Wyclif. The author expects his survey of this literature will bring to "light views hitherto unknown to historians" (p. xiv)—historians in general, yes, but Wyclif students, no. Vaclav Mudroch did not extend his study to the nineteenth and twentieth centuries because most of the later biographies of Wyclif were done by Protestant clergymen who had little new to offer, while the more scholarly writing was by "historian-scientists" who wrote for members of their own profession.

Among the writers of the sixteenth century whom Mudroch introduces are the Dominican Bernard of Luxembourg, John Bale who hailed Wyclif the "morning star," and John Foxe whom the author refers to as "the only trustworthy guide to Wyclif's and other martyrs' lives for the English Protestant for many years to come" (p. 10).

There was more about Wyclif from seventeenth-century writers and it was usually offered with greater detachment. Among these writers were Nicolas Vignier, Robert Bellarmine, and Thomas James. James sought to tailor his treatment of Wyclif to make him acceptable to Anglicans who could not stomach what the Oxford schoolman had written about dominion and disendowment and who, along with other Protestants, puzzled over his ambivalence on the nature of the Eucharist. A vigorous debate between the Frenchman Antoine Varillas and Gilbert Burnet, bishop of Salisbury, who denounced his adversary's latest work on Wyclif as the "falsest coyn that can be struck" (p. 41), attested to the continued interest in the English reformer.

The eighteenth century, the "Age of Reason," treated Wyclif with still greater objectivity but had much less to say. Religious toleration had brought in its wake growing indifference to the doctrinal controversies that had fired earlier generations. Apart from his Protestant apologists, the few writers who did notice Wyclif were ironically the *philosophes*, who were apt to praise anyone who had run afoul of the medieval church. The one Protestant writer deserving of particular mention was John Lewis, who produced the best biography of Wyclif yet to appear.

The author, an émigré Czech whose life ended before he saw his study in print, came to his interest in Wyclif via the reformer's influence on Jan Hus and his Bohemian contemporaries. Mudroch must have been an inspiring teacher. Albert Compton Reeves, one of his devoted students, edited this study. The volume offers unusually extensive notes, a bibliography, and an index.

JOSEPH H. DAHMUS  
*Pennsylvania State University*

PATRICIA JAMES. *Population Malthus: His Life and Times*. Boston: Routledge and Kegan Paul. 1979. Pp. xv, 524. \$43.50.

After nearly two decades of painstaking effort, Patricia James has provided us with her biography of T. R. Malthus, the first full account of the life, times, and works of the great economist. James is to be commended not only for having undertaken the task of searching out the scattered materials upon which she bases her study but also for setting down what appears to be a reliable and well-written account of Malthus's life to replace the few short and error-laden essays upon which readers have previously had to rely.

The author has thoroughly immersed herself in the period. Given the sparseness of material concerning Malthus directly, she has filled the book with information concerning most of the people of any importance whom he knew, the places he lived in or visited, and the ideas with which he came into contact. She also seems to have quoted in part, and frequently entirely, every one of Malthus's surviving letters. While this certainly sets the scene, the reader at times comes to believe he has been perhaps too generously provided for.

Ideally, Malthus's biographer ought to have a good understanding not merely of economics but of the history of economic thought. In both these respects, we are somewhat let down. We do not get a satisfactory sense of what Keynes had in mind when he complained that the triumph of Ricardo's system over that of Malthus had "constrained the subject for a full hundred years in an artificial groove." Although James agrees, not being an economist she is unable to provide a persuasive defense of this view. Although she does provide us with a good understanding of the *Essay on Population* and with some sense of the uproar that greeted its publication, she is less able to provide a satisfactory account of the much-neglected but important *Principles of Political Economy*. Such critical questions as the debate on Say's Law, for example, are passed over all too superficially.

Still her book will prove useful to historians of economic thought. Her discussion of the relationship

of Malthus and his foremost disciple, the Scottish clergyman-economist Thomas Chalmers, displays a socially inept Malthus neglecting the Scotsman's admiring letters and thereby causing great offense. Reading the correspondence, we can be persuaded that such faults were indeed obstacles to the formation of a Malthusian school to rival that of Ricardo. But when the author provides the same explanation for McCulloch's profound distaste for Malthus's economics, she is less convincing. She does not explore the meaning of Malthus's system as a full social and economic outlook, which would alone sufficiently account for McCulloch's antagonism as well as for the later fierce and at times scurrilous attacks of Marx, who never waited in vain for a letter of reply from Malthus.

In England biographers and historians have generally been placed into two separate classes, and it may be thought unfair to judge James by the standards of the latter. As a biographer, she writes effectively and engagingly. She has certainly performed useful service in collecting the facts of Malthus's life and in correcting the errors that have become almost fixed. But Malthus was first of all an economist, and the reader will find the book's treatment of economic questions disappointing. Perhaps the publication of this fine biography will stimulate the writing of a thorough treatment of the Malthusian system.

BERNARD SEMMEL  
*State University of New York,  
 Stony Brook*

MICHAEL RUSE. *The Darwinian Revolution: Science Red in Tooth and Claw*. Chicago: University of Chicago Press. 1979. Pp. xv, 320. \$20.00.

In 1910 the geologist J. W. Judd told "the story of a great revolution in science" in a textbook entitled *The Coming of Evolution*. Seventy years later Michael Ruse, a philosopher of science, has furnished students with a similar account, drawing on recent Darwin scholarship to tell the "story" of how "the organic origins problem" was solved in Great Britain between 1830 and 1875.

It is gratifying to find a contemporary philosopher of science taking a detailed interest in his nineteenth-century counterparts, and for this reason *The Darwinian Revolution* deserves serious consideration from historians interested in John Herschel and William Whewell. As a philosopher Ruse has also studied the works of his story's other dramatic personae, among them Charles Lyell, Adam Sedgwick, Richard Owen, T. H. Huxley, and the hero, Charles Darwin. A discerning reader will not allow the author's evident dislike for some of his characters, and his unabashed admiration for others, to

detract from the enduring value of his philosophical discussions.

As narrative history, on the other hand, the book may well interest students chiefly because much of the writing is qualitatively indistinguishable from their own compositions. Granted the merits of a racy style in reviewing complex theoretical problems for lay persons, there is still no excuse for Ruse's surfeit of clichés, mixed metaphors, and historical howlers. Feathers are ruffled, woods are missed for trees, and cake is had and eaten on more than one occasion. Darwin's barnacle books are "pregnant with veiled hints" about evolution, and Ruse's own narrative is structured around "threads" that "throw light" on the course of events. "The 1860s were a very different time from the 1840s" when, according to another context, time did not stand still. Herbert Spencer (1820–1903), who "was Victorian in more things than sex," "filled volume after volume as only nineteenth-century writers could." Et cetera. "Essentially," as Ruse is fond of saying, the writing and editing bear many marks of haste.

More serious still, the story's plot is as deficient as its style. Ruse never attempts to justify the term "revolution," presuming evidently that readers will find metaphors as unproblematic as he does. The "Darwinian Revolution" is taken loosely as "the whole series of ideas and events leading up to and away from" the *Origin of Species* in 1859 (pp. ix–x), a "movement" whose scientific, philosophical, religious, and sociopolitical "strands" are "intertwined" but ultimately distinct and separable, as indeed they are treated in the book. The movement gathers momentum after "the hero" enters the scene in 1830—the scientific community was "moving toward evolutionism"; natural and revealed religion were "preparing the way," even "unconsciously"; Darwin was following "his path to discovery"—and the movement retires like Darwin himself, exhausted and triumphant, in 1875.

None of which is a great advance on Judd's historiography of seventy years ago. But it reminds the critic, ironically, of the shrewd judgment of the philosopher who observed, "Nothing is ever quite so straightforward as it appears in textbooks" (p. 65).

JAMES R. MOORE  
Open University

NEAL C. GILLESPIE. *Charles Darwin and the Problem of Creation*. Chicago: University of Chicago Press. 1979. Pp. xiii, 201. \$16.50.

Drawing on the work of Michel Foucault and Thomas Kuhn, Neal C. Gillespie examines Darwin's theory of evolution in terms of the epistemic crisis and paradigm shift from "creationism" to "positivism" in nineteenth-century Anglo-Ameri-

can science. Gillespie's essay demonstrates that an understanding of the Darwinians must take full account of the fact that they lived in an age of not one but two conflicting epistemes, each invoking different standards of scientific knowledge and theories of nature. More significantly, Gillespie argues that the Darwinian epistemic shift was not sudden or discontinuous; to the end of his life Darwin continued to "promote one episteme while never breaking free of another" (p. 7). This fact makes the study of Darwin and his age fascinating and yet complex and resistant to simplistic classifications.

Two chapters examine the views of the Anglo-American scientists who continued to hold to some notion of special creation or, under the growing influence of positivism, retreated to "scientific nescience" concerning the true causes of species origination and examine how, for Darwin's developing positivism, all of these became dangerous obstacles to scientific advance. Two further chapters detail Darwin's *scientific* attack and his *theological* objections in the *Origin* to special creation, teleology, and nescience. The scientific arguments have been rehearsed many times, but Gillespie's treatment of Darwin's theological preoccupations underlines the fact that Darwin's career and historical role are more complicated and ambiguous than is often recognized. "To read the *Origin* solely as a positivistic work . . . is to reinterpret it from a post-Darwinian perspective" (p. 124).

Darwin's "double vision" is evident in his continuing worry over the moral implications of the waste and cruelty of nature. Forced to break the link between the moral and natural world, Darwin paradoxically sought to preserve it as well. The cost involved removing God from nature and denying any idea of a divine providence. God's relation to the world became increasingly inexplicable—that is, impossible for Darwin—yet, as Gillespie shows, into the 1860s and 1870s "elements of the creationist and positivistic epistemes coexisted in Darwin's mind in a loose, paradoxical and curiously unantagonistic way" (p. 125).

The heuristic use of the concepts of "episteme" and "paradigm change" helps illuminate both Darwin's personal development and the move in Victorian science toward positivism. However, the principal value of this study lies in Gillespie's demonstration that there was indeed a *real* conflict of Victorian science and religion and that the conflict was over *ideas* as well as over political power and leadership. Gillespie underscores the unsoundness of the contention of some historians—for example, Owen Chadwick—that "no enquiry in the realm of the physical could produce results in the realm of the spiritual." The Darwinian epistemic shift was an event of incalculable significance for religion. Although it is unquestionably true that the



Darwinian revolution did not require the surrender of religion as such, Gillespie leaves us to ponder further his simple yet suggestive remark: "That religion could continue under such terms often concealed from participants what had actually occurred" (p. 153).

JAMES C. LIVINGSTON  
*College of William and Mary*

MICHAEL ADAS. *Prophets of Rebellion: Millenarian Protest Movements against the European Colonial Order*. (Studies in Comparative World History.) Chapel Hill: University of North Carolina Press. 1979. Pp. xxvii, 243. \$19.00.

Studies of millenarianism have assumed the proportions of an industry in recent years, but few of the emerging articles and monographs have been genuinely comparative; most anatomize a single case or string several together without exploring links between them. Thus Michael Adas's *Prophets of Rebellion* is an especially welcome contribution, for it explicitly compares five quite different non-Western movements: the Java War of Dipanagara's forces against the Dutch (1825–30); the Maori Pai Māreua movement (1864–ca. 1867); the Birsa rising in East-Central India (1899–1900); the Maji Maji rebellion in German East Africa (1905–06); and the Saya San rebellion in Burma (1930–32).

The selection accomplishes several goals at once. It makes accessible to American scholars information on movements relatively little studied outside Europe (the Java War and Pai Māreua have been notably slighted in the past). The five also include three different colonial regimes—Dutch, British, and German—as well as a century of colonial experience during which the character of European penetration changed. Finally the cases are dispersed over a geographical-cultural arc incorporating diverse economies, social structures, and topographies.

Adas has wisely chosen to avoid merely stringing cases together. Save for introductory narratives of each that provide a frame of reference, the book is structured around themes addressed to all five: causes, mobilization, impact, and so forth. In the course of the analysis, Adas displays a truly impressive grasp not only of the historical materials but also of social scientific literature as well. Other works on non-Western millenarianism may have encompassed a larger number of movements; none has been as carefully structured.

If anything negative may be said, it is that the theoretical perspectives tend to be applied in a less intensive manner than might be desired. The range of themes is so large that few receive as full a treatment as they require. Thus, the section on causation is based upon a relative deprivation model, and, al-

though Adas is clearly aware of the criticisms directed against relative deprivation, neither the model nor the evidence is presented in sufficient detail to ease misgivings. On the other hand, there are striking and original discussions one wishes had been prolonged. The analysis of relationships between millenarian prophets and their more bellicose subordinates illuminates our knowledge of the ambiguous links between leaders and followers and of the roots of violence in millenarian uprisings. The discussion of that tangled subject—the rationality of non-Western societies—is as balanced as it is insightful. These finely wrought "islands of theory" (always, by the way, related to the data) are at once a delight and a frustration—clarifying while at the same time crying out for more extended treatment. One area that does receive full development is the discussion of magic as a device for neutralizing European military power. The phenomenon of talismans against bullets (repeated in North America during the Sioux "Ghost Dance") fascinated earlier writers but was often merely the occasion for a recitation of exotic anecdotes. Here the matter is explored with a refreshing combination of sympathy and intellectual rigor.

Since all five instances ended in defeat, there is a temptation to categorize such eruptions of discontent as (in E. J. Hobsbawm's dismissive phrase) "pre-political." As Adas suggests, however, they varied widely in the military challenges posed as well as in the amelioration of conditions that followed defeat. And in the end what commends this superb study is precisely the rare accommodation of both uniqueness and generality, building a broad, cross-cultural understanding of millenarian movements without sacrificing the special circumstances of each instance.

MICHAEL BARKUN  
*Syracuse University*

MICHAEL HOWARD, editor. *Restraints on War: Studies in the Limitation of Armed Conflict*. New York: Oxford University Press. 1979. Pp. viii, 173. \$16.95.

Michael Howard's introduction to this fine collective work repeats some points made in his *War in European History* (1976). Western Europe's now "traditional" restraints on war began with agreements by the seventeenth-century's "new" monarchies to limit—in organizations that possessed "exceptional sanctions for the enforcement of obedience"—the actions of their own armed forces. The solution to the destructiveness of revolutionary and religious wars was their prevention. The destructiveness of colonial war was to be mitigated by the conquerors' Christian consciences rather than by international law.



As described in paralleled chapters by Geoffrey Best, Bryan Ranft, and D. C. Watt, the chief problem before 1945 was to adjust the resulting rules of land, sea, and air warfare to protect civilians and neutrals against the increasing power and range of the new weapons of the Industrial Revolution. War itself was "outlawed" in 1928. Less than two decades later a series of international tribunals found particular individuals guilty of crimes against peace and humanity and of belonging to political organizations that had conspired to commit those crimes.

By making 1945 the turning point of their book, the authors may inadvertently reinforce the common Western view of the problem as one of restraining military technology. Though deterrence itself is not a legal problem, it has made many other legal problems ambiguous. Nobody knows, John C. Garnett remarks, whether conventional weapons can be used in an unlimited way in a bargaining process that stops just short of nuclear war or whether such weapons are also subject to new restraints, such as those that prevented attacks on Vietnamese dams, which, under World War II rules, were quite legitimate targets. Laurence Martin's chapter on limited nuclear war points to other ambiguities. Using "tactical nuclear weapons to achieve a militarily meaningful demonstration of resolve" has to be discussed because soldiers and politicians may not think it a wholly fanciful topic.

D. P. O'Connell's discussion of war at sea since 1945 also suggests that many of the new restraints on sea power stem less from the traditional sources of maritime law than from those changing concepts of war criminality by which, G. I. A. D. Draper remarks, all self-declared wars of national or racial liberation are "just" and their perpetrators incapable of criminality. Whether the new revolutionaries will, like Europe's new monarchies, eventually adopt rules to protect themselves from later revolutionaries is an open question. Meanwhile both the developed black hats and the underdeveloped white hats have savaged almost every possible "Rule of the Humanitarian Law of Armed Conflict."

Nitpicking so short a work on such a major topic is relatively easy. Almost everyone will have some quarrels with the lists that each author has suggested for further reading. But this work is so well done on a major issue of historical and human concern that it is far more important to recommend it for careful and thoughtful reading.

THEODORE ROPP  
Duke University

#### ANCIENT

ELLIS RIVKIN. *A Hidden Revolution*. Nashville: Abingdon. 1978. Pp. 336. \$12.95.

This deeply personal and religious statement of the definition and meaning of the Pharisees, a sect in ancient Judaism, emerges from nearly four decades of reflection, including two of active research. The Pharisees are presented as "a class of audacious revolutionaries." Their principal doctrine concerned a two-fold revelation of God to Moses at Mount Sinai, part in writing, part orally formulated and transmitted, and, later on, taught by the Pharisees themselves.

This thesis is worked out through a careful study of the three principal sources that refer to the Pharisees, passages of the Gospels, allusions in the historical writings on Josephus, and sayings, tales, and fables in the latter rabbinic literature about sages who lived before the destruction of the Temple of Jerusalem in A.D. 70. Rivkin analyzes the information in these several sources and then undertakes a historical reconstruction. The third part of the work is his synthesis of the results of the thoroughgoing analysis of the sources. The Pharisees were "a scholar class, championing the twofold Law (Torah) and enjoying great power and prestige"—that is the sum and substance of the matter. From this definition, Rivkin proceeds to ask when the Pharisees came into existence. It was in 280 or 180 B.C., and the book of Ben Sira, which represents a "pre-Pharisaic and pre-twofold Law society" indicates the date must be the later of the two. The chief result of this Pharisaic revolution was to incorporate within Judaism "major structural components and major conceptual notions prevalent in the Greco-Roman world." The book is written vividly, but with control and taste. It is a model of first-class historical argumentation and exposition. The author's personal conviction of the authority for his own religious life of the facts he relates in no way intervenes in his lucid mode of thought. What is advocated is a historical position, and, only secondarily, a pious evaluation of that position.

The principal difficulty is that the allegations of all the sources constitute the starting point for argument and analysis. The literary problems to be solved before the sources are to be believed as fact simply are not taken up. The fact, for example, that the Mishnah and the later rabbinic writings come four or more centuries after the events of which they speak and the men to whom they attribute sayings, does not stop Rivkin from using them any way he wants. True, Rivkin recognizes that the Mishnah comes long after the Pharisaic "revolution" has run its course. But the character of the Mishnah's evidence for that "revolution" nonetheless is taken to be definitive. Josephus is cited as a totally accurate reporter of events. The same is so for the other sources assembled to form the book's thesis. It would not be fair to call the work wholly gullible and credulous, let alone fundamentalist. Rivkin

takes a position quite independent of his sources. But the position is built out of essentially unanalyzed sources. The problems of using those sources for historical reconstruction are not systematically and rigorously confronted. So for Rivkin, the sources present facts, and the facts define the problem. In my view, the sources themselves constitute the first and principal problem. Still, this fundamental difference of opinion on method in no way prevents recognizing Rivkin's imaginative and often profound contribution to the interpretation of Pharisaism as a religious movement.

JACOB NEUSNER  
Brown University

NOBERT BROCKMEYER. *Antike Sklaverei*. (Erträge der Forschung, number 116.) Darmstadt: Wissenschaftliche Buchgesellschaft. 1979. Pp. xv, 392.

Few historians doubt that slavery played a fundamental role in Greco-Roman antiquity, but its study has been far less important in historical writing than its position in ancient society seems to warrant. A volume that attempts to summarize and synthesize modern research into ancient slavery—even one that fails to make a major intervention in current debates—is therefore to be welcomed.

Norbert Brockmeyer's *Antike Sklaverei* is an extended research report. It begins with a relatively brief overview of research methodologies and perspectives, focusing on problems of definition, current research problems, and general viewpoints regarding the place of slavery in ancient society. Much attention is given to a supposed seminal division between Marxist and non-Marxist work. A much longer section follows, providing a more detailed, chronological overview of slavery and related institutions from the Bronze Age to the later Roman Empire.

Brockmeyer provides the reader, especially the nonspecialist reader, with much useful bibliography, some idea of the varied research projects currently being pursued (his sections on Eastern European work and that of the Mainz Academy will be of special interest to English-speaking readers), and a good deal of useful information on particular aspects of slavery. The bibliographical notations, the brief summaries of individual positions, and the analyses of recent work on ancient source material are not always as critical or informative as they might have been, but such failings are perhaps unavoidable in a relatively brief survey of so large and varied a subject.

The work's conceptual failings are more disturbing. The counterposing of Marxist and non-Marxist perspectives, which at first sight seems rational and even progressive, breaks down upon closer in-

spection. Marxist historians of slavery have hardly been so unified in their points of view as to form a single, easily defined grouping, and Brockmeyer's tendency to view Marxists as uniformly eastern European is not helpful. Nor is work done within a broadly Marxist tradition necessarily sharply antithetical to other research; the essays of M. I. Finley, for example, demonstrate the fruitful possibilities of a sophisticated synthesis of non-Marxist and Marxist points of view.

For all his emphasis on Marxism, Brockmeyer has not completely assimilated an essential element of the Marxist tradition. Although he does recognize the importance of the Marxist insistence that empirical research is inadequate when unaccompanied by a broad focus on the general role of slavery within ancient society as a whole, he fails to carry this insight through in this volume. His definitions of slavery seem inadequate, and the wider social position of slavery tends to be forgotten; what is too often substituted is a rather eclectic sampling of research on particular points, which are left unconnected and without a broader context. Chapters that do attempt the broader project, those on the Roman Empire (chaps. 12–14), are the best in the book and hint at what might have been accomplished elsewhere.

These conceptual failures derive, of course, to a large extent from the state of the field itself. It is surely impossible to summarize the "facts" of a field in which there is so little agreement on the very nature of the main problems and their relationships to the wider questions of ancient history. Brockmeyer might well have written a better book had he devoted it entirely to the question of research perspectives and current controversies over the very nature of ancient slavery, better delineating the strengths and weaknesses of the various points of view and showing more precisely where the field actually stands at this moment. This task remains to be accomplished.

ROBERT A. PADGUG  
New York City

ROBIN SEAGER. *Pompey: A Political Biography*. Berkeley and Los Angeles: University of California Press. 1979. Pp. 209. \$25.00.

The career of Pompey the Great is a major puzzle in the complicated history of the Late Republic, the best-illuminated era of ancient history. First came his meteoric rise to a triumph in his twenties, against the disapproval of the fearsome dictator Sulla. Then he was hatchetman for the Senate in Italy and Spain, consul with Crassus in 70, and thereafter a leading figure until his fatal duel with Caesar.

Robin Seager, senior lecturer at Liverpool, has written a close-packed survey of the era, well buttressed with references to sources and modern studies but too detailed in fact to appeal to a beginning student. While one may differ from his view on any one disputed point—and there are many in the period—his assessments are always reasonable and carefully argued. His tale, however, is an oft-told one, recently by Erich Gruen in *Last Generation of the Roman Republic* (1974); the main problem still remains, namely, what were Pompey's own goals and plans?

The name of Pompey is often introduced in Seager's pages, but one never does get a clear view of the man himself. By implication the view arises that Pompey, by and large, reacted to each turn of politics on its own, seeking to retain his position as leading citizen of Rome, anxious to have men's good opinion, really afraid of the dangers of assassination. In his last pages Seager more directly supports one ancient judgment "that Pompeius could tolerate no equal in power and esteem, Caesar no superior" (pp. 186–87) but also cites the poet Lucan's inability "to make up his own mind where the truth lay."

Why this ambiguity? Caesar spoke for himself in his commentaries; Cicero's views are abundantly evident in his speeches and letters; Pompey, alas, is only a man, in the mirror especially of Cicero's work, who almost never stands alone on the stage. Beryl Rawson, parenthetically, has recently surveyed the uneasy relations of Cicero and Pompey in *The Politics of Friendship* (1978). Maybe Pompey did not wish to speak out clearly, for he was a man who rose through war, not politics; but this seems a judgment of unnecessary despair. J. van Ooteghem wrote a 600-page study of Pompey that J. P. V. D. Balsdon summed up (*Journal of Roman Studies* [1955]), "as concerns Pompey and the problems about Pompey, the book leaves things very much as they were before its publication." Much the same can be said of the present work, which could have been better edited (for example, repetition of facts and footnotes on pages 2 and 4, 36 and 38) and more carefully proofread. There is a good bibliography and an adequate index of proper names.

CHESTER G. STARR

*University of Michigan,  
Ann Arbor*

THOMAS N. MITCHELL. *Cicero: The Ascending Years*. New Haven: Yale University Press. 1979. Pp. xii, 259. \$17.50.

Since other biographies favor the well-documented years from 63 onwards, Thomas N. Mitchell's monograph on Cicero's early life fills a real

need. Four long chapters analyze Cicero's background and upbringing, his long apprenticeship under such figures as Lucius Licinius Crassus and the two Quincti Mucii Scaevolae, his advance up the ladder of magistracies, and his consulship. While Mitchell leaves aside the technical development of Cicero's rhetoric, he portrays Cicero's formation as a politician and political thinker against the background of socioeconomic developments that affected the late Roman Republic. He uses prosopography carefully and takes a judicious position on the question of factions among the ruling elite.

On the other hand, Mitchell's view that the admitted strife for honors, *dignitas*, and influence within the nobility "was almost constantly overshadowed by a more basic concern to maintain the aristocracy as a whole in a stable position of power in the face of growing threats" (p. 19) lays too much stress on aristocratic unity. The disruptive ambition and opportunism of men like Marius, Sulla, Cinna, Lepidus, Pompey, Crassus, Catiline, and Caesar should not be seen as less significant, but as symptomatic of aristocratic politics in the late Republic, even in the 90s, which Mitchell views too rosily. Naturally, the nobles pre-eminent at a given time sought to preserve the status quo in the face of disruptive changes, but some aristocrats were always willing to exploit those changes in order to advance themselves, so that the aristocracy lacked the degree of unity necessary to treat problems adequately or retain control.

Overemphasizing aristocratic unity also leads to a distorted interpretation of Cicero's political ideals. Mitchell believes that Cicero's ideas reflected the interests and policies of the oligarchy and "may also safely be regarded as representative of the conservative *nobilitas* as a whole" (p. 197). That Cicero's conceptions of the Republic and the ideal aristocrat were molded by association with Lucius Crassus and the Scaevolae does not mean that the rest of the nobility equally shared these ideals. The men about whose higher thoughts anything is known are revealed primarily through Cicero, who, naturally, presents a sample biased in favor of those with whom he sympathizes. While a majority of nobles would have shared Cicero's concept of their rights, they are less likely to have shared his view of their duties. Although limited, Cicero's vision of the *via optima* went beyond that of most contemporaries.

On many other points Mitchell's analysis is judicious and astute: compare Sulla's march on Rome, Cicero's attitudes towards Cinna and Sulla, and the trial of Verres. After his treatment of the notorious Rullan land bill no more need be said. Similarly valuable is the discussion of the *Senatus Consultum Ultimum*. The social and economic background of the Catilinarian Conspiracy is deftly filled in, and the presentation of Catiline, his actions, and his motives

is difficult to improve. Clearly, this book is required reading for those interested in Cicero and the late Roman Republic.

ALLEN M. WARD  
University of Connecticut

J. H. W. G. LIEBESCHUETZ. *Continuity and Change in Roman Religion*. New York: Oxford University Press. 1979. Pp. xv, 359. \$49.00.

This book is well written and its thesis well researched. Beginning with the era of the late Roman Republic, J. H. W. G. Liebeschuetz traces the development of chosen aspects of Roman belief, both religious and philosophical, down to the fourth century. He emphasizes philosophical problems of morality and indeed gives particular attention to stoicism as a kind of religious creed.

Because of his choice of evidence, however, the author gives scant attention to nearly two centuries of the period he treats. In his introduction (p. xv) Liebeschuetz admits partiality in his choice of evidence. He selects from and follows the writings that give expression to the "religious attitudes of the Roman establishment . . . worth studying even in isolation." From what has he isolated these ideas, many of which were alien to a Roman's notion of religion? Finding "it necessary to be selective," he has paid little heed to the bulk of the evidence on Roman religion, namely the religious lore nested in antiquarian authorities and the mass of epigraphic documentation recording the religious sentiments of high- and low-born Romans alike. The reader may be somewhat puzzled when the author discusses the general absence of inscriptional evidence occurring in the later third century since he has received no indication of the nature and extent of that precious material for the rest of period under discussion.

What Liebeschuetz has achieved is an intellectual history of selected literary authorities' views on some religious practices and moral beliefs. The authorities represent both learned disquisition (Cicero, Seneca, Arnobius, Lactantius) and belletristic endeavors (Lucan, Silius Italicus, Tacitus, Pliny the Younger, Apuleius) but none reflects the regular cult practices of Roman religion of all social classes. Besides treating moral problems, Liebeschuetz gives due space to such matters as magic and astrology.

Liebeschuetz's contribution lies in the theme of social crisis, political reform, and attendant beliefs affirmed or newly adopted by government to foster religion and morality. His treatment synthesizes many works of predecessors in promotion of this thesis. Having chosen a course of considerable selectivity, Liebeschuetz cannot claim to have exhausted his subject or, for that matter, to have dealt with Roman religion. On the other hand, he has suc-

ceeded in what he set out to do. Continuity and change are so well handled in terms of the intellectual history of some religious beliefs and moral problems that on reaching the conclusion the reader recognizes the coherence of the subject from polytheistic paganism to monotheistic Christianity.

ROBERT E. A. PALMER  
University of Pennsylvania

HILDEGARD TEMPORINI. *Die Frauen am Hofe Trajans: Ein Beitrag zur Stellung der Augustae im Principat*. Berlin: De Gruyter. 1978. Pp. xii, 295. DM 112.

The two parts of this book amass the evidence for the spouses of Trajan and Hadrian, Pompeia Plotina and Ulpia Marciana respectively. Each part begins with family origins, continues with an elaborately documented narrative of the woman's career, and concludes with a lengthy discussion of her deification. There are full indexes of ancient sources, proper names, and modern authors together with selected illustrations and a stemma.

The foreword (p. ix) advises that the book began its life as a doctoral dissertation. Traces of that genre remain. Thus one notes gratuitous displays of learning in the bare *PIR* references to Livia (p. 27 n. 110) or Gaius and Lucius Caesar (p. 36 n. 178). Again, the notes frequently present long lists of modern treatments of the topic under consideration without any indication of the different viewpoints of those modern treatments. Undigested ancient evidence also appears, such as the long lists of coins (pp. 100-04); one notes in passing that numismatic evidence is usually far less reliable than the author's use of it would imply. Such accumulations of evidence ancient and modern often seem tangential: one questions the relevance for the author's topic of so much data for consuls from the provinces (pp. 20-21).

Footnote virtuosity per se does not constitute the quarrel with this book. Rather, one questions the tacit assumption prevalent among ancient historians that their craft consists of offering an annotated anthology, as it were, of facts. Description, of course, has an important role, but not at the expense of interpretation. And the false emphasis on facts ultimately distorts the sources it purports to treat faithfully. For example, it is incapable of filtering out the various unconscious distortions that ancient authors may have injected in their writings. Again, the only questions description asks are those that the ancients seem to suggest—these do not necessarily represent the most helpful questions. Keith Hopkins's penetrating remarks in this regard (*JRS*, 68 [1978]: 178-86) acutely demonstrate how a preoccupation with facts produces essentially ersatz history; my review agrees with his strictures.



Interpretation must go beyond discussing points of evidence. It should probe the relationships between those points and their larger social contexts. Consider the deification of the two women in Temporini's book. What was the social constituency for those deifications? Would a *colonus* be as impressed as a senator, and for what reasons? In a male-dominated society one wonders about the socioreligious import of such elevations. How did they affect the power structures of either gender? The book's methodological innocence never raises such questions.

Temporini has produced, in effect, a series of factual surveys of two imperial women and their spouses. As such it marks a valuable start. But the book is description, not history. It is sad that ancient historians so seldom recognize that one of the founders of their craft, Herodotus, emphasized (1.5.3-4) the necessity both of facts and interpretation in the historical enterprise.

CHARLES ROBERT PHILLIPS III  
*Lehigh University*

TIMOTHY E. GREGORY. *Vox Populi: Popular Opinion and Violence in the Religious Controversies of the Fifth Century A. D.* Columbus: Ohio State University Press. 1979. Pp. xii, 245. \$20.00.

"No wild beasts are such enemies to mankind as are most Christians to each other." Timothy E. Gregory's book substantiates only too well these sardonic words of the fourth-century historian Ammianus. His theme is the religious controversies of the fifth century and the violent popular involvement therein. As illustration, Gregory analyzes four episodes: the episcopacy of John Chrysostom; Nestorius and the Council of Ephesus; the Latrocinium (or Robber Council); and the Council of Chalcedon.

The selective method has its dangers, chronological restriction in particular. The fifth century was not unique. Donatists in Africa or the riots in Constantinople under Anastasius (for easy example) could equally well have been exploited for a book on this theme. Nevertheless, Gregory is on balance right to confine himself to specific cases. The last thing the world needs is another tissue of generalities on the nature of violence.

Which is not to say that Gregory was wrong to offer some by way of introduction and conclusion. Historical texts, especially the sort he has to work from, do not always speak for themselves to the professional scholar, let alone the general reader whose attention this book fully deserves. Gregory's summarizing remarks are admirably level-headed. He is the slave of no "ism," rejecting the panaceas of Marxism and trendy sociology in favor of the plain facts of Byzantine history and human nature.

People, uneducated and otherwise, are easily seduced into violence when they have few outlets for legitimate protest. Religion afforded particular impetus since a man's personal salvation depended on both himself and the state getting their beliefs right.

A few details are inaccurate. The regular number of circus factions in the early Empire was four, not six (p. 28)—six was a temporary aberration under Domitian. The celebrated riot at Ephesus was not a popular one (p. 31): *Acts* 19: 23-28 shows that only the vested interests of the silversmiths were involved. The account of Chrysaphius's execution (pp. 166 and 194, n. 14) ignores Malalas's statement that he was liquidated because he was a prominent Green.

There are some trifling misprints: "unfortunately" (p. 96), "increased" (p. 157, n. 62), "Theophenes" (p. 186). The bibliography has the merit of being designed for use rather than ostentation (though J. R. Martindale's thesis [p. 239; also p. 14, n. 14] is an Oxford B.Litt., not a B.A.). The index, unfortunately, is too exiguous and arbitrary to be of much service.

The style is on the stodgy side, albeit mercifully jargon-free. But Gregory, thanks to his enviable mastery of a jumble of disparate and desperate sources and to his sanity of judgment and clarity of exposition, has written an important book on a subject that, as recent events from Ireland to Iran show, remains all too timely.

BARRY BALDWIN  
*University of Calgary*

## MEDIEVAL

JAMES MULDOON. *Popes, Lawyers, and Infidels: The Church and the Non-Christian World, 1250-1550.* (The Middle Ages.) Philadelphia: University of Pennsylvania Press. 1979. Pp. xi, 212. \$15.00.

As indicated by the title, this book deals with the intercultural contacts between Christian Europe and its non-Christian neighbors between 1250 and 1550. These Christian-infidel relations were conducted according to the theories and rules of canon law, which authorized or justified all such interaction for popes and Christian kings alike.

Innocent IV, a noted canon lawyer before he became pope, is the central figure in this study because he fused legal theory and papal practice. This influential pope and canonist believed that Christians could and should live in peace with their non-Christian neighbors and that the infidels had natural rights to land and their own government—they could not legally be deprived of their property and freedom simply because they were not Christians. Innocent IV saw the entire world in ecclesiastical terms, and to him all infidels were potential mem-



bers of the Church. He believed that the papacy had the right to send peaceful missionaries into all non-Christian societies and that military attack was justified if the infidels refused to accept these missionaries. Pope Innocent IV thus favored a policy of sending religious embassies to achieve peaceful relations with infidels. His Asian mission stressed reaching the Mongols by means of conversion; most of his letters to the Mongols are religious in nature and stress conversion to Christianity, not diplomatic and military alliance. Thus, in accord with the long tradition of Church-State conflict in Western Europe, Innocent IV and later canonists concluded that, for spiritual reasons, the papacy could intervene at will in all infidel societies just as it could in secular affairs.

In the succeeding centuries, Innocent IV's views on the legal rights of infidels in the face of Christian contact and expansion were generally continued. Even the Avignon popes were actively involved in relations with infidels, as revealed by letters in the Papal Registers. These letters are still basically religious, and many are nothing more than a reissue of previous papal pronouncements.

In the fifteen and sixteenth centuries, secular states began to replace the papacy as the leader of European society and, in the process, papal and canonist influence began to be pushed into the background. The Christian conquest of the Atlantic Islands and the Americas was considered inevitable, and the papacy emerged as the mediator as to whether Portugal or Spain would be in charge of protecting the peaceful Christian missionaries converting the hostile primitive natives. But after a religiously and legally correct conquest, the economic, political, and diplomatic interests of Portugal and Spain took precedence over the legal rights of the infidels and the views of the papacy. When this happened, European intercultural contact had passed beyond its medieval canon law framework and had entered the era of modern international law; secular rather than religious factors would determine how Christians related to their non-Christian neighbors.

This book is a well-documented study. The two basic sources are the papal letters of the thirteenth and fourteenth centuries and the writings of the canon lawyers of the fourteenth and fifteenth centuries. Much of the material is drawn directly from these two sets of primary sources. The footnote and bibliography references are solidly grounded in canon law studies as one would expect in a legal study of this nature. It is only when the author reaches beyond his canon law core of expertise that one notices omissions in his bibliography. He spends a considerable amount of time talking about the Mongol mission initiated by Innocent IV and continued by subsequent popes, but one fails to find

any of the numerous articles on this subject by Denis Sinor and Jean Richard. Their most important articles of the last four decades have been conveniently assembled and published by Variorum Reprints. Also conspicuous by their absence are the books of Gian Andri Bezzola, *Die Mongolen in abendländischer Sicht (1220-1270)* (1974), Jean Richard, *La papauté et les missions d'Orient au moyen âge (XIII<sup>e</sup>-XV<sup>e</sup> siècles)* (1977), and William Urban, *The Baltic Crusade* (1975). Recent works by the above authors would have added to the material on the Mongols and Lithuanians, although the last Richard volume may not yet have been available when this manuscript was originally submitted for publication.

The author's extensive work and publication in the area of medieval canon law make him well qualified to write this legal study. While others have dealt with medieval Europe's relations with various non-Christians from the viewpoint of one specific people or geographic area, this volume is the first to consider them all and to study and analyze these relations from the viewpoint of a common and continuous legal base. By going back to the canon law theory behind late medieval Christian-infidel contact and practice, this comprehensive legal study makes a valuable contribution to the understanding and legal justification of later European expansion in the age of exploration and discovery.

GREGORY G. GUZMAN  
Bradley University

PIERRE RICÉ. *Les écoles et l'enseignement dans l'Occident chrétien de la fin du V<sup>e</sup> siècle au milieu du XI<sup>e</sup> siècle.* (Collection Historique.) Paris: Aubier Montaigne. 1979. Pp. 462. 96 fr.

Pierre Riché's earlier book on medieval education, *Education et culture dans l'Occident barbare* (1962), one of the most impressive investigations into early medieval history in our time, has a worthy sequel in the present work. Like its predecessor, this study is based upon an extremely thorough winnowing of narrative, diplomatic, juridical, epigraphical, and archeological sources and provides a richer and more detailed portrait of the evolution of medieval education than we have previously had.

After an introduction summarizing his earlier work on the end of ancient education and the beginning of Christian schools, Riché treats successively of schools during the Carolingian Renaissance, schools from the middle of the tenth century to the middle of the eleventh century, masters, students, curriculum, and methods, and the education of the lay person. These discussions are followed by fifty pages of extracts from texts that illuminate particular points, twenty chronological tables that harmonize religious, political, and educational

events in various parts of the medieval West, an extremely thorough bibliography of secondary literature, an index to authors in the bibliography, and a general index of names.

The period that Riché has chosen to examine is an age that produced remarkably few works on educational theory and practice. Previous historians of education have generally preferred to use a few singular individuals (Cassiodorus, Isidore of Seville, Gregory the Great, Alcuin) as steppingstones across the turbid barbarian flood and have thus grossly underestimated the extent and richness of educational practice in the early Middle Ages. Riché's weighing of the slightest clues from sources as disparate as civil and religious legislation, monastic rules, lives of saints, and library catalogues forces scholars to re-examine the genesis and uniqueness of the twelfth-century Renaissance, just as his earlier work dimmed somewhat the luster of the Carolingian Renaissance. He here contends that "entre le VII<sup>e</sup> et le XI<sup>e</sup> siècle les conditions de l'enseignement n'ont guère changé" (p. 6) and demonstrates the validity of his view with masses of evidence not previously utilized. He has done so without resorting to the expedient of defining education so broadly as to include virtually all of life and culture; for him, education means masters and students, teaching and learning, curriculum and methods, books and study. He has done so without muddling together lists of evidence from different regions and times; tight chronology and geographical discrimination are characteristic of his work. He has done so without minimizing the impact of individuals; the index lists about six hundred contemporaries mentioned in the book. And he has done so without slighting the often-ignored subject of the education of lay persons; a fascinating section deals with the education of aristocrats both in Latin literacy and vernacular culture, and with the religious education of the laity through the liturgy, catechesis, preaching, and songs.

Riché's previous history of medieval education waited fourteen years for publication in an English translation. It will be a shame if the present work must wait so long.

JOSEPH M. MCCARTHY  
*Suffolk University*

NATALIE FRYDE. *The Tyranny and Fall of Edward II, 1321-1326*. New York: Cambridge University Press. 1979. Pp. x, 301. \$25.00.

Natalie Fryde has written an excellent and most stimulating book soundly based both upon well-known chronicles and upon a wide range of record sources hitherto incompletely investigated. Now we have a revised interpretation of Edward II's later years comparable to those produced in recent years

for the middle period of his reign by J. Maddicott and J. R. S. Phillips.

The results are interesting and significant. Contrary to Stubbs's opinion that Edward was the first English king since the Norman Conquest untrained and uninterested in government business, Fryde demonstrates that an almost pathological greed for money (due to inherited debts and the poverty of his earlier years) drove him to a ruthless supervision of the Exchequer officials. Before his deposition he had accumulated a reserve equal to about one year's income. Though this was a considerable achievement, it is perhaps something of an exaggeration to describe such sums as "enormous treasure."

This seems to have been Edward's own personal achievement, and Fryde refutes Tout's contention that the famous Exchequer reforms of the 1320s were due to the Despensers. Deprived of even this achievement, there is nothing left but complete condemnation for this atrocious family and their hangers-on. The Despenser dominance of government in the 1320s must surely have been one of the most bestial (if not the most bestial) regimes in English history. The Despensers terrorized almost every social group in the land—their neighbors in Wales and the Marches, the Lancastrians, the city of London—and yet, as a result, lived in a nightmare of fear themselves. They pursued their victims with a vindictiveness hitherto unknown in English politics. Thomas of Lancaster's execution was unprecedented. Before this aristocrats had been executed only for collaboration with enemies of the realm. The prolonged imprisonment of nobles and their families, including children and elderly relations, perpetrated quite a new brutality in political relationships. Devastation, plundering, and the breakdown of law and order in many parts of the country were, on the evidence that Fryde produces, far, far worse than anything that occurred during the Wars of the Roses. It is little wonder that Isabella and Mortimer were able to conquer with an invasion force of only 1,500 men.

Fryde also deals with the story, based upon the famous letter of the papal notary, Manuel de Fieschi, that Edward II escaped from Berkeley Castle and ended his life in Italy. Unfortunately discussion of this problem is a little superficial in comparison with the rest of the book. This section needs to be read in conjunction with Tout's paper on the same subject (*The Collected Papers of T. F. Tout* [1934]) and the recent article in *Speculum* (July 1978) by C. P. Cuttino and T. W. Lyman that obviously appeared too late for Fryde to make use of.

Except for an excessive liking for passive verbs, this book is very well written. I strongly recommend it both for its style and its scholarship.

J. R. LANDER  
*University of Western Ontario*

RICHARD H. ROUSE and MARY A. ROUSE. *Preachers, Florilegia and Sermons: Studies on the Manipulus Florum of Thomas of Ireland*. (Studies and Texts, number 47.) Toronto: Pontifical Institute of Mediaeval Studies. 1979. Pp. xi, 476. \$24.00.

Medieval sermon studies are increasingly receiving the attention and recognition they warrant. This book by Richard H. Rouse and Mary A. Rouse is an outstanding contribution to the growing number of works enhancing our appreciation of this field. In 1306, Thomas of Ireland, fellow of the Sorbonne, published the *Manipulus Florum* (sheaf of extracts). As "an alphabetically-arranged topical compendium of *auctoritates*, designed for use in writing sermons" (p. ix), the *Manipulus Florum* was a typical product of its age. Consisting of some 6,000 extracts from patristic and ancient sources, classified according to 266 topics, the *Manipulus Florum* (including prologue and bibliography) would correspond to a modern work of 365 pages. Its audience was the average university-trained preacher who needed a useful reference manual for sermons that were becoming increasingly complex in structure and dependent upon the citation of authorities. The usefulness and popularity of the work are attested by 180 surviving manuscripts and some 47 printings between 1483 and 1887 (described in the appendix). Further, the *Manipulus Florum* became the vehicle whereby twelfth- and thirteenth-century sources were made available to many later writers including not only preachers but also theologians, mystics, lawyers, and vernacular poets.

The authors give initial attention to placing the text in its thirteenth-century background and demonstrate how aids to preaching, such as distinctions, concordances, and subject indexes, were linked to the growing importance of preaching. We learn here, for example, that medieval Europe had an effective biblical concordance only in the 1280s. New techniques had to be adopted: developments of book and page layouts and the emergence of reference systems, including the adoption of Arabic numerals and the acceptance of alphabetical order as a means of arranging words and ideas. The authors correctly stress the importance of alphabetization of materials (Thomas of Ireland's significant contribution) and point out that this was not necessarily in accord with the medieval view: "to discuss *Filius* before *Pater*, or *Angelus* before *Deus*, simply because the alphabet required it, would have seemed absurd" (p. 35).

Part 2 contains biographical material on Thomas of Ireland and a treatment of the structure, sources, and history of the *Manipulus Florum*. Thomas of Ireland intended his bibliography, the most elaborate of its type in this period (and here edited with the prologue for the first time), to be used in con-

junction with the original sources. This anthologist wanted to encourage his readers to go beyond the extracts to the complete works. For the medieval preacher whose library resources were scanty, the *Manipulus Florum* served a most important purpose. We are indebted to Richard and Mary Rouse for this fine study of a medieval compendium that, unoriginal as it was, is, nonetheless, an important key to the intellectual life of the age.

PHYLLIS B. ROBERTS

College of Staten Island and  
Graduate Center,  
City University of New York

PIERRE DESPORTES. *Reims et les Rémois aux XIII<sup>e</sup> et XIV<sup>e</sup> siècles*. Paris: Éditions A. and J. Picard. 1979. Pp. 743.

This work treats every possible aspect of the history of the town of Reims: society, economics, and politics (both ecclesiastical and secular), as well as monumental-topographic subjects. It is principally a history of the old *civitas* and immediately adjacent areas. Other parts for which there remain little or no documentation, notably the bourg of Saint-Rémi, are perforce somewhat neglected. The work is divided into three large chronological sections. That from ca. 1150 to 1270 was one of rapid growth marked by international commerce, especially in linens. The period from 1270 to 1338 represented the height and maturity of the community, and that from 1338 to 1435 was marked by population decline, intermittent warfare, and the gradual absorption of this archiepiscopal town by royal control. Pierre Desportes adduces pertinent analogies from other towns in France and elsewhere to illuminate the history of his town.

In terms of population, the author believes that the town reached its peak of about twenty thousand around 1270. The heavy mortality of the years from 1316 to 1320 reduced it to about sixteen to eighteen thousand in 1328. The plague of 1347 took about a quarter of the population and ushered in the troubled period of the later Middle Ages. A remarkable aspect of the population of Reims was the heavy proportion of clergy, about 12 percent. Reims was an archiepiscopal see, its prelate was its prince, and there were no less than three *officiales* with their own courts in town. A result was a relatively high percentage of clerks specializing in law and scribal services. Furthermore, a certain percentage of the ordinary population of the community was, although living in an altogether secular manner, in clericalure and therefore relatively exempt from ordinary taxation. This, together with the continued independence of the bourg of Saint-Rémi, helps to explain why Reims never really defeated the power of its ecclesiastical lord. Lastly, the Jewish popu-

lation was tiny after the first half of the twelfth century.

Because it is impossible to mention all the subjects described in the author's pages, I shall touch on one or two. As elsewhere in Europe, the teens and twenties of the thirteenth century saw an attack on usury both ecclesiastical and popular. Having profited from the old usurious investments, the well-to-do were obliged to change their tune from the 1230s on. They turned to house rents, census contracts, and, to some degree, contracts with interest as a penalty for nonfulfillment. At the same time, an oddity of Reims is that the crafts and trades never became as significant in this period as they did elsewhere, and the patrician families were more or less able to retain stable control of the community.

The important town families were in origin not dissimilar to those in other older *civitates* in western Europe. They were partly knightly and partly burgher, the former being more closely linked to the archbishop than the latter, who had won some self-government by the *Willelmine* charter granted by the archbishop-prince in 1182. The author asserts that the knightly or noble families tended to move out of town in the period around 1300, just before the beginnings of ennoblement for selected burghers who had served the king. The weakness of the documentation here makes the treatment of this subject not quite satisfying. In regard to the burgher patricians, also, the author asserts that the possession of a hereditary patronymic was a "privilege" of old families. I would not have used the word "privilege," but Desportes amply proves that patrician lines regularly employed family names generation after generation. The same was not true, or cannot be proved, of the crafts- and tradesfolk, and the author surmises that a reason for this is that because of emigration, higher mortality, and so forth, humbler families were not liable to have as long a life.

In fine, this is a good book that raises many problems and solves not a few. It is indispensable for those interested in urban history.

JOHN HINE MUNDY  
Columbia University

CONSTANCE BRITAIN BOUCHARD. *Spirituality and Administration: The Role of the Bishop in Twelfth-Century Auxerre*. (Speculum Anniversary Monographs, number 5.) Cambridge, Mass.: Mediaeval Academy of America. 1979. Pp. viii, 144. Cloth \$11.00, paper \$5.00.

Constance Britain Bouchard's *Spirituality and Administration: The Role of the Bishop in Twelfth-Century Auxerre* is based on the author's dissertation. It should be noted, however, that the dissertation itself

must still be consulted for documents from the archives of Auxerre that were edited by Bouchard in an appendix to the dissertation. The published monograph discusses the lives of the bishops of Auxerre from 1092 to 1220. The chief source for the description is the *Gesta Pontificum Autissiodorensium*, a continuous series of episcopal biographies for Auxerre from the ninth century to the thirteenth. The chapters of Bouchard's book are chronologically arranged, and one each is devoted to the seven biographies of the *Gesta* that she used in the edition of L.-M. Duru: Humbaud (1092-1114), Hugh of Montaigu (1115-1136), Hugh of Mâcon (1136-1151), Alain (1152-1167), William of Toucy (1167-1181), Hugh of Noyers (1183-1206), and William of Seignelay (1207-1220). In most of these chapters, Bouchard provides some additional information from episcopal charters and from a second independent and contemporary narrative source from Auxerre, the Chronicle of Robert of St.-Marien. When possible, the biographers' bias is evaluated in the light of these outside sources.

It is Bouchard's intention to illustrate the twelfth-century change in the understanding of the concepts of spirituality and administration in the lives of the bishops of Auxerre. She concludes that "over the course of the twelfth century spirituality clearly experienced a relative decline in importance among the qualities desired in a bishop" (p. 142). Spirituality is used by Bouchard in the narrow sense of the word, meaning "the religious dimension of the inner life, that which tends towards God" (Bouchard, p. 14, and A. Vauchez, *La spiritualité du moyen âge occidental, VIII<sup>e</sup>-XII<sup>e</sup> siècles* [1975] pp. 5-7. Bouchard misunderstands Vauchez's use of the terms "broad" and "narrow" spirituality.) The virtues and the degree of sanctity that the biographers attributed to the individual bishops in the *Gesta* provide Bouchard with the basis for her estimation of their spirituality, but she does not seem to feel very comfortable with the concept. She notes that by the time of William of Seignelay (1207-1220), "spirituality and administration were . . . essentially one" (p. 143). This astonishing conclusion is merely explained by references to the practically exclusive interest of William's biographer in the bishop's administrative abilities and his admiration for these skills. It is quite true that, as Bouchard points out in her introduction, "by the end of the [twelfth] century there were multiple perceptions of the spiritual life, with different religious goals for different orders of society" (p. 15), but this is not the same thing as saying that administration came to equal spirituality. The *Gesta*, valuable as they are, do not seem to provide sufficient material to analyze something as personal as an individual's inner life or his religiosity, let alone to show how a bishop's religiosity expressed itself in his everyday activity. Bouchard,



who seems to be attracted more by social and quantitative history than by intellectual history, appears not fully aware of the dimensions of her subject, for she never notices that the biographer of Humbaud (1092–1114) “saw a bishop’s excellence as residing not so much in his spirituality as in his administrative accomplishments” (p. 35). This anonymous writer had, therefore, the same attitude toward spirituality and administration as his successor of more than a hundred years later, the biographer of William of Seignelay.

There are some further criticisms: Bouchard usually fails to cite relevant critical editions of primary sources—bibliographical information is at a premium—and the volume is marred by numerous typographical errors. Nevertheless, Bouchard’s book is a welcome addition to the relatively small number of volumes available in English on medieval French history. One may not learn much about “spirituality,” but the monograph brings to life the complexities and difficulties that were faced by bishops in twelfth-century Auxerre. Despite certain shortcomings, therefore, *Spirituality and Administration* is a good book to have. It brings the unusual wealth of information for twelfth-century Auxerre to general attention and should stimulate further research.

UTA-RENAME BLUMENTHAL  
Catholic University of America

THOMAS F. GLICK. *Islamic and Christian Spain in the Early Middle Ages*. Princeton: Princeton University Press. 1979. Pp. xi, 376. \$22.50.

The interaction and interpenetration of Islamic and Christian societies in the Iberian peninsula during the three hundred years following the initial Muslim invasion is the subject of this book. Thomas F. Glick, whose study of irrigation in medieval Valencia is well known, has directed his inquiry to various aspects of social and cultural contact, arguing against a much older notion that medieval Spain was divided into watertight compartments that did not permit Muslims or Christians to affect one another’s lifestyles other than in a superficial manner. In addition, he takes up the challenge laid down by Américo Castro and Claudio Sánchez Albornoz and gives a perceptive critique of the methods followed by each of them. He has advanced the argument much beyond the point where they left it and makes it clear that the comparatively simplistic view of medieval Spain advanced by them, each from his own position, is no longer tenable.

After an introductory chapter in which he discusses the Castro–Sánchez Albornoz polemic and the movement of ideas and techniques over political frontiers and other barriers, he divides his book into two parts. The first treats the social and economic

development of early medieval Spain, and the second concentrates on the diffusion of techniques and ideas. Among the many topics studied, all from a comparative point of view, are agricultural production, irrigation practices, the movement away from local herding of sheep to transhumance, the use of forest land, the growth of towns and markets, trade and investment, bonds of kinship, social classes, the intermingling of ethnic groups, the process of translation, and the transmission of technological and scientific knowledge from East to West.

In every instance, Glick subjects old assumptions to an intense criticism. So, for example, he questions the assumption that al-Andalus was particularly prey to political instability, indicating that Islamic society was tribal and segmentary and that a highly centralized state was not the norm to be expected in Islamic Spain. Stability was provided in fact by the law, which transcended political boundaries. He notes also that no satisfactory explanation has been given for the fall of the caliphate of Córdoba but suggests that it was a model that no longer suited the needs of society, especially in view of the intense Berber immigration that preceded it. Similarly, he argues that the model of towns in Christian Spain was Islamic, rather than the Germanic *burgum*, whose establishment along the pilgrimage route to Santiago de Compostela has been studied by Valdeavellano. There are, however, some gratuitous assumptions that are accepted without further inquiry, as for example, “the borrowing by Christians of the Islamic *ribât*, or fighting ‘monastery’ and its re-invention as the crusading military order.” The history of the *ribât* in Spain and the Holy Land and its relationship to the military orders, if any, has yet to be studied in depth.

As an Islamist, Glick tells us that his view of Spain may well differ from that of others, especially medieval Hispanists. Indeed this is true, but there is no doubt that all of us are greatly indebted to him for holding the peninsula up to a new light and showing us new facets of this many-faceted jewel.

The apparatus includes several figures and tables and a map illustrating such things as the ratio of Christian and Muslim scientists. There are also a bibliographical essay and footnotes.

JOSEPH F. O’CALLAGHAN  
Fordham University

JIRÍ SPĚVÁČEK. *Karl IV: Sein Leben und seine staatsmännische Leistung*. Prague: Academia, and Hermann Böhlau Nachf., Vienna. 1978. Pp. 208. DM 80.

The six-hundredth anniversary in 1978 of the death of Charles IV, Emperor of the Romans and King of Bohemia, prompted several important historical exhibits and a number of scholarly publications. Most of this activity took place in West Germany and ex-



pressed a view of Charles's work in which the Bohemian or indeed Czech element was only one among others. The book under review is a useful statement of the Czech position (it has been translated from a Czech original) by a scholar who has come to history by way of diplomatics and whose work is correspondingly precise, solidly based on documentary sources, and on the whole rather austere. It does not attempt to set Charles in the richly textured world of late-Gothic Europe, nor does it offer a systematic narrative of public action; it is instead a tightly packed essay designed to present us with a Charles IV who was independent, original, stubborn, creative, and inspired above all by the Czech and Slavonic traditions that were his by right of his mother.

In his drive to become ruler of Bohemia, Charles had to overcome the indifference or hostility of his father; in his acquisition of the German crown ("King of the Romans") and consolidation of his position in the Empire, he had to work against the Wittelsbachs and later against the wishes of the Avignon papacy; in his struggle to create an effective royal government in Bohemia, in line with the Přemyslid tradition, he had to fight the barons of the land. His overall aim was to create the forms that would give permanence to what he himself had achieved: an empire held together by an emperor who was also King of Bohemia, with both the imperial and Bohemian titles hereditary in the Luxemburg family, and with "Bohemia" expanded into the massive territorial conglomerate that had been put together by the Přemyslids, Charles's father John, and Charles himself: it included—along with Bohemia and Moravia—Silesia, Lusatia, Brandenburg, and a number of other territories.

Such non-national dynastic conglomerates were common late medieval phenomena, but they cannot be grasped, conceptually, by categories drawn from the post-medieval world of national states. Charles's apt concept, "The Lands of the Bohemian Crown," was a politico-legal invention that synthesized "Western" ideas of the crown and Slavonic notions of a historical tradition going back to the Great Moravian Empire. All of Charles's work to exalt Bohemia and Prague, religiously, culturally, and politically, was undertaken in the service of this program. In the end he himself began to lose his grip as a result of intractable realities and his own physical decline; his eldest son and successor, Wenceslas IV, turned out badly, and the more competent Sigismund failed to produce a son. (He did however have a daughter, who carried the Luxemburg legacy to the Habsburgs, who made it work until 1918.)

All in all a stimulating, extremely useful book—but with no index, no bibliography, and no map.

HOWARD KAMINSKY  
Florida International University

THOMAS LINDKVIST. *Landborna i Norden under äldre medeltid* [The *Landbor* (Tenants) in the Nordic Countries in the Early Middle Ages]. (Studia Historica Upsaliensia, number 110.) Uppsala: Uppsala University; distributed by Almqvist and Wiksell International, Stockholm. 1979. Pp. 179. 80 KR.

When Marx analyzed the precapitalistic economy in the medieval period, he postulated a feudal mode of production closely tied to the political and juridical control that the privileged classes exercised over the peasants in the form of serfdom. Only in this generation have Marxist scholars argued that economic and not political-juridical criteria should be the prime determinants in a materialistic conception of history. Historians such as Perry Anderson, Witold Kula, and E. A. Kosminskij have pointed out that the phenomenon of large estates was essential for the feudal mode of production. At the same time, sociologists like Barry Hindess and Paul Hirst have provided theoretical models for a rigorous conceptualization of Marxist theory. The crucial problem for these authors is to explain how the nonproducers were able to appropriate the surplus from the producers, since the latter were in control of both the means of production, the land, and the production process.

The present study, a doctoral dissertation from the University of Uppsala, fits this framework. Thomas Lindkvist compares the tenant farmers in the Scandinavian countries during the early Middle Ages, here defined as the period from the early twelfth to the early fourteenth century. Since both serfdom and large estates were, with some exceptions, absent, this area offers an opportunity to seek the salient feature of the feudal mode of production. The author finds this in the feudal rent that took the form of labor, produce, or money. In two central chapters, Lindkvist provides a structural analysis of the patterns of landholding among the tenant farmers and of their juridical position. His evidence comes from letters, charters, land surveys, and, mainly, laws that he sees as normative. In spite of chronological and regional differences, he points to the common features that distinguished the Scandinavian tenant farmers from their European contemporaries. They were personally free and, in most respects, legally equal to the landowners, but they disposed of their land only for short periods of time. This did not necessarily mean that they were forced to move regularly, but that the contract with the landowners had to be renewed periodically or, in many places, annually. In other words, because of the peasants' legal position and the tenuous nature of their ties with the land, the feudal mode of production in Scandinavia did not bind them to the soil, but had the opposite effect. By increasing the obligations for each renewal of

the contract, the landowners were able to reap the benefit of a swelling agricultural productivity. In a more speculative and less successful final chapter based mainly on literary evidence, Lindkvist argues for a double origin of tenant farmers, by ascent from the sizable slave population found in the Viking period and by descent from the independent freeholders, pauperized by the simultaneous appearance of church and state. Tenant farmers have been studied in individual Nordic countries, mainly by Russian and East European scholars. Lindkvist's work is therefore useful as the first comprehensive and comparative study of this subject. The book has a good English summary.

JENNY M. JOCHENS  
Towson State University

DONALD M. NICOL. *Church and Society in the Last Centuries of Byzantium: The Birkbeck Lectures, 1977*. New York: Cambridge University Press. 1979. Pp. 162. \$19.95.

The impressive development of Byzantine studies in the past decades makes increasing specialization and greater technicality of most publications inevitable and, indeed, desirable. But it is also good that Byzantinists of Donald M. Nicol's stature occasionally produce a discussion of major issues involving not only the Byzantine Empire of the Palaeologi, but also Eastern Christendom as a whole, its survival in the Middle East, in the Balkans, and in Russia after the fall of Byzantium, and its relations with the West. Until recently, if Byzantium received any credit from historians of the Middle Ages and the Renaissance, it was for its reputation as an upholder of Greek antiquity and its preservation of the writings and ideas of ancient authors for transmission to posterity. What Nicol shows is that Byzantine society, on the eve of its demise, was not so much concerned with preserving Hellenism, as with "its spiritual identity that was nourished by an irrational belief in the interdependence of time and eternity, a sense of belonging to a theocratic society" (p. 130).

This does not mean, however, that the Byzantine society of the fourteenth and the fifteenth centuries lacked intellectuals who read Plato and Aristotle, or that Byzantine copyists did not actually preserve ancient texts, but that these concerns belonged only to a few and were generally considered by the Church and the state as a lower priority when compared with theological orthodoxy and a religion emphasizing personal experience and spiritual continuity. The controversy that opposed the hesychast monks to representatives of intellectual humanism and that ended in 1347-1351 with the victory of the monks stands at the center of Nicol's discussion of Byzantine society, and his judgment on its results is

both categorical and balanced: the "outer wisdom" of Greek antiquity was not forbidden or suppressed (it still found eminent adherents like Gemisthos Pletho and Bessarion), but "the growing influence and authority of the Orthodox Church as the guardian of the true Byzantine conscience meant that Hellenic learning was relegated to a secondary place" (p. 65).

Does this mean that old Gibbon was right in characterizing Eastern Rome as nothing but a despicable hotbed of "barbarism" and "religion"? This, clearly, is a question of taste and conviction, which Nicol does not want to answer directly. Actually, he remains sympathetic to Byzantine Christian civilization for what it really was and avoids foreign criteria of judgment.

One of the best living specialists in late Byzantium, Nicol is both brilliant in his presentation and accurate in his references. His book can be used as the most reliable and complete source on late Byzantine intellectual history. It also contains useful lists of Byzantine emperors and patriarchs of Constantinople between 1261 and 1453, with exact chronological data for each, and an excellent bibliography. Not only Byzantinists but also Western medievalists and historians of Eastern Europe will find this book to be an accurate and intellectually stimulating presentation of Byzantium's "last will" to posterity.

JOHN MEYENDORFF  
Fordham University and  
St. Vladimir's Theological Seminary

## MODERN EUROPE

CATHARINA LIS and HUGO SOLY. *Poverty and Capitalism in Pre-Industrial Europe*. Translated by JAMES COONAN. (Pre-Industrial Europe, 1350-1850, number 1.) Atlantic Highlands, N.J.: Humanities Press. 1979. Pp. xvi, 267. \$26.25.

Catharina Lis and Hugo Soly argue that poverty was a structural feature throughout the *ancien régime*: the extent and character of poverty in economic backwaters seems similar to that in areas of economic boom, in periods of population stagnation to that of periods of growth. Without completely denying the altruism of the rich and powerful, they also argue that the primary aim of preindustrial poor relief was the control of public order and of the labor supply. Both of these premises are, by now, accepted historiographic canon. The real contribution of this syncretical survey is to show their constant and interdependent relationship.

Mediterranean and East European phenomena are discussed when appropriate, but much of the book is, in fact, an extended comparison of French

and English developments with a healthy grounding of examples drawn from the authors' native lowlands. The book consists of five chapters, ranging chronologically from "Feudalism, Poverty, and Charity, c. 1000–c. 1350" to "Economic Growth, Pauperization, and the Regulation of the Labour Market, c. 1750–c. 1850." The "long sixteenth century," animated by the triumph of commercial capitalism, and the period 1750–1850, by the growth of industrial capitalism, receive special attention. Each chapter follows a three-part structure: the causes of socioeconomic inequality among peasants and urban craftsmen; the living and working conditions of the poor; and finally the responses of political and economic elites to these threatening poor.

The authors persuasively argue that "'polarization' perhaps better than any other word characterizes the social evolution of early modern Europe" (p. 71). This polarization is reflected in the physical development of cities, the separation of high and low culture, and, in the mid-sixteenth century, a poor relief renaissance in the policies of some sixty European towns and cities. They also provide an excellent survey of the "Great Confinement" of the seventeenth century, seeing it not only as a French phenomenon but generalized throughout the rest of Europe as well.

The authors conclude that the "trinity of charity-control-labour regulation" (p. 222) is the inescapable reality of private preindustrial poor relief. As the effects of industrial capitalism increased, so did a whole series of paternalistic programs—private and public—further heighten the polarization characteristic of the earlier period.

This well-written work includes helpful charts and maps, complete notes, and critical bibliographies for each chapter. It offers a synthesis that, unlike previous excellent research on poor relief and charity (Gutton, Mollat, and others), also thoroughly relates poverty to the total economic development of preindustrial Europe. As the first in this "Pre-Industrial Europe" series, it sets a standard for those that follow.

HOWARD M. SOLOMON  
Tufts University

JEAN-LOUIS FLANDRIN. *Families in Former Times: Kinship, Household and Sexuality*. Translated by RICHARD SOUTH-ERN. (Themes in the Social Sciences.) New York: Cambridge University Press. 1979. Pp. xi, 265. Cloth \$32.50, paper \$8.95.

The nearly simultaneous publication of Jean-Louis Flandrin's opus and those of Stone, Shorter, Pillorget, Donzelot, and Foucault established family structure, familial sentiment, and sexuality as major themes of eighteenth-century historiography. In the

first of two loosely linked parts, Flandrin lays important groundwork for the field by disentangling the multiple definitions of family, the several systems of reckoning kinship, and the regional varieties of household structure and inheritance in early modern France. Drawing widely on personal papers, customary codes, and secondary case studies, Flandrin rescues the profusion of extended family types, especially characteristic of southern France, from the leveling tendencies of Laslett's median of means and from the myopia of his own colleagues in the Paris Basin.

The second part is more idiosyncratic. From confessional literature Flandrin concludes that the values and behavior of the modern family issued from a complex dialectic set in motion by an internal contradiction in the post-Tridentine Church: the simultaneous valorization of family life and commitment to a celibate clergy. The Church's internal contradiction, according to Flandrin, had murderous consequences: sexual repression, the greatest infant mortality in Europe, and the inability of the French to adjust population size to available resources as the English did. Eventually French elites, first in Europe to adopt the sentimental family and birth control because they were the most acutely repressed, acted to realize half the Church's message—the ideal of family life—by rejecting its limitations on sexuality.

The book is as testy as it is versatile. It juxtaposes French family reconstitution to English census demography only to probe the weaknesses of each. It subtly champions southern communal families over those infected by the "neurotic individualism" of the north. Flandrin contends that the Sin of Onan was proliferating, first in extramarital and then in marital relations. He makes the case that women initiated contraception, able to do so because the Church's ethic of marital reciprocity increased female autonomy.

Flandrin's brand of flamboyant conjecture makes family history something like Montgolfier's balloon: propelled by its own dynamism, the course of its flight is little affected by the contours of the landscape on which others plod, and where on that terrain it might eventually touch down is anyone's guess. But, after all, the landing—the anchoring of family history to political and institutional history—is crucial to the flight's success. Although Flandrin's preface justifies the historian's concern with the private past on the grounds that domestic life was (and is) intertwined with public conflict, he fails to relate his narrative to public developments "in former times": to tax policies, legal changes, power shifts, economic changes, all of which contributed to the transformation of families that is his theme.

Nor is the book the synthesis of recent studies for

the general reader it claims to be. Highly technical, it is rendered nearly unintelligible by a careless presentation. This makes the book something like the Sin of Onan itself: a pleasant stimulation and potentially fertile but so scattershot that much of its spirit falls fallow.

CAROLYN C. LOUGEE  
Stanford University

ANDRÉ CORVISIER. *Armies and Societies in Europe, 1494–1789*. Translated by ABIGAIL T. SIDDALL. Bloomington: Indiana University Press. 1979. Pp. ix, 209. \$15.00.

The thesis of André Corvisier's masterly book is that the "military mentality" was more pervasive in early modern Europe than many twentieth-century historians have assumed. He examines militarization under three headings: the army's relations to the nation, the army as an arm of an increasingly centralized state, and the army as a society with its own system of recruiting, promotion, ethics, and so forth. Much of Corvisier's previous work was based on his statistical examination of existing military records. In this one he uses his mastery of those records to examine the army's changing place in a changing society.

This book has already been recognized in France as a modern military-historical classic. There is hardly a topic upon which it does not have something interesting to say. It is least clear on "militarization" or the "military permeation of society" as part of the process of modernization. This is not surprising. Until "feudal" and "modern" and active and passive citizens' attendant military rights and duties have been more clearly defined, it will be hard to assess the novelty of the army's "new" position in society.

Corvisier's sections on the growth of military administration and military nobility in the service of the state are particularly interesting because of his comparative approach. He accepts the traditional Western view of Prussia as the exemplar of the military state and sees East European societies becoming more military as they followed the lead of the West's new monarchies. By 1789 Western Europe remained an area of "military social groups within but distinct from society as a whole." This view carries over to the very dubious conclusion that the militarization of society "set off by the French Revolution" was interrupted in the early nineteenth century and that the "nearly omnipresent militarization . . . of 1914 came about only with the adoption . . . of universal military service by societies that were no longer military in nature." The peacetime nations in arms of 1914 were thus militarized but not military, although they were soon to devote all

of their efforts to a Napoleonic goal of total military victory that left most of them in ruins in 1945.

This caveat suggests that contemporary historians may still be trying to define military, militarism, and militarization. It should not keep anyone from reading and pondering this splendid, well-translated work. As the title indicates, the naval side of military power is not dealt with, even for Great Britain, the United Provinces, and Portugal, which is not considered at all.

THEODORE ROPP  
Duke University

MICHAEL BALFOUR. *Propaganda in War, 1939–1945: Organisations, Policies and Publics in Britain and Germany*. Boston: Routledge and Kegan Paul. 1979. Pp. xvii, 520. \$37.50.

The standard German nationalist line in the 1920s was that Allied, especially British, propaganda had been in large part responsible for the outcome of the Great War. Hence a book comparing British and German propaganda in the Second World War immediately whets the appetite, particularly one from the pen of a distinguished British civil servant and historian of Germany who spent the years between 1939 and 1945 involved in the "war of words." In fact no one could be more qualified for this venture than Michael Balfour.

The subject matter is fraught with pitfalls. On the one hand there are the musty volumes of the *Völkische Beobachter* from whose odors many a well-intentioned researcher has been known to expire. On the other hand there are sufficient succulent morsels to tempt the weak-willed to try and join the ranks of the best sellers. Balfour has come through the dangers splendidly. His sources are a cross section of government materials, intelligence reports, and published documents, and his choice of topics for discussion is always eminently sober. A ripe tale of heroism like the raid on St. Nazaire in March 1942 is dispensed with in a chapter of three pages; and a titillating rumor such as the scare about the Germans dropping rattlesnakes on Britain from airplanes receives less than a sentence.

In the first third of the book Balfour outlines the organization of propaganda in the two states, demonstrating particularly effectively the degree of confusion and the intensity of rivalries in the Nazi propaganda apparatus. In the second part, the bulk of the book, he presents the responses of the British and German governments to certain specific events and issues during the war. The German side receives a fuller treatment, ostensibly because of the availability of primary source material. Brendan Bracken did not keep a diary; Joseph Goebbels did. As the reverses set in after 1942 Goebbels finally



achieved greater control of the propaganda effort and, much impressed by the attitude of Churchill after Dunkirk, urged a "propaganda of the truth." This did not, however, in the end alter the general impression of rashness and inconsistency left by German propaganda, in contrast to the restraint and basic continuity of British pronouncements.

How did propaganda affect the course of the war? The answer Balfour gives is, of necessity, ambiguous. He recognizes that propaganda is bound to reflect the nature of a society and its government, and hence he is inclined to say that propaganda did not affect the actual military outcome of the war. Nevertheless he has assembled plenty of evidence that it was not altogether ineffective either. The problem is that one cannot with any kind of accuracy assess the effect: the sources for such assessment are slim and not very reliable. In sum, this book is an excellent history of the organization and implementation of propaganda in the Second World War, and for the rest Sisyphus would be proud, because the next time the stone should roll much more easily.

MODRIS EKSTEINS  
*University of Toronto*

PENRY WILLIAMS. *The Tudor Regime*. New York: Clarendon Press of Oxford University Press. 1979. Pp. xii, 486. \$43.00.

In this book Penry Williams, a fellow of New College, Oxford, and specialist in the history of Wales in the sixteenth century, has set out to describe the ways in which the Tudor government actually worked. This is constitutional history but written with an emphasis on men rather than institutions. Williams is especially concerned to relate the central government to the localities, to discuss the actual operation of policy and legislation, and to evaluate the effectiveness of Tudor administration.

These are worthwhile goals. Most traditional studies of Tudor government have concentrated on the monarchy, Parliament, and the institutions of central government without paying much attention to the question of local enforcement. More recently, as local history has gained in academic respectability, a number of studies of specific cities, counties, and regions have appeared, but there has been little attempt to integrate the parts and the whole. The matter is especially important because many historians have argued that the Tudor regime was feeble and ineffective and that Elizabeth in particular failed to deal with urgent issues, thus bequeathing a welter of unsolved problems to the Stuarts. Williams acknowledges that he shared these views when he began to write, but he now believes the adverse judgment to be mistaken. The Tudor govern-

ment, he argues, had limited objectives, and it succeeded quite decently in achieving stability, controlling violence, preventing famine, and encouraging trade. All of this was accomplished with a remarkably small administrative force: there was one royal officer for each four thousand inhabitants, while the bureaucrat-ridden French had one for each four hundred.

Williams's work is marked by its tone of moderation and common sense. In the ongoing debate about Tudor despotism, for instance, he occupies a middle ground between Hurstfield's gloom and Elton's whitewash. A short section on Marian religion is valuable in suggesting the positive activities of an administration that is usually remembered only for its exiles and executions, and a brief chapter on rebellions stimulates thought by classifying rebellions into types, showing that protests cannot be related directly to bad harvests and arguing that insurrections actually strengthened the central government because they provided grounds for the punishment of government enemies.

If the book has a problem, it lies in the question of its intended audience. Most of the material will be familiar to specialists, since this work is based on published studies rather than original archival research, yet Williams presupposes too much knowledge and too serious interest for the volume to suit novices. Specific errors are few. The statute of wills did not actually repeal the statute of uses (p. 349), although it did modify its operation significantly. It is not really "surprising" (p. 426) that the Marquis of Winchester seldom attended the Elizabethan Privy Council, since he was a septuagenarian and no longer robust by the time of the queen's accession. The author of Sir Thomas Smith's biography is Mary, not Margaret, Dewar (p. 297). There is no bibliography, but footnotes are particularly full and demonstrate an extraordinarily complete use of recent monographs, articles, and dissertations. In sum, the book provides a useful, thought-provoking synthesis.

STANFORD E. LEHMBERG  
*University of Minnesota,  
Twin Cities*

RALPH HOULBROOKE. *Church Courts and the People During the English Reformation, 1520-1570*. (Oxford Historical Monographs.) New York: Oxford University Press. 1979. Pp. viii, 304. \$29.00.

In recent years the sixteenth-century ecclesiastical courts have received well-deserved attention: proceedings have been edited (E. M. Elvey, *The Courts of the Archdeaconry of Buckingham* [1975] and M. Bowker, *An Episcopal Court Book for the Diocese of Lincoln* [1967]); the courts' part in maintaining the faith



and correcting the failings of the laity and clergy has been examined (R. A. Marchant, *The Puritans and the Church Courts* [1960] and *The Church under the Law* [1969]); and some progress has been made in assessing their popularity and utility (S. Lander, "The Church Courts and the Reformation" in *Continuity and Change*, ed. R. O'Day and F. Heal [1976] and M. Bowker, "The Commons' Supplication," in *Transactions of the Royal Historical Society*, 21 [1971]). Many questions remain, not the least of which is how the very institutions, which were so vigorously attacked at the outset of the Henrician Reformation in the *Commons' Supplication Against the Ordinaries*, survived the century with the same powers (if not the same influence) and with the same law, although the study of that law had been prohibited in 1535.

It is against this background that Ralph Houlbrooke's book must be welcomed. He has studied two dioceses, Norwich and Winchester. He shows how both instance and correction procedure were conducted (as did B. L. Woodcock in *Medieval Ecclesiastical Courts* [1952]), and he indicates a decline in both types of business, at least in Norwich. But his conclusion that "lasting damage" was done to the courts in the 1530s (p. 38) is questionable in view of his statistics on the increase in cases before them. What changed was not the use but the status of the courts within society: ecclesiastical censure ceased to carry such opprobrium, and alternatives had to be found—and frequently were found—to the pre-Reformation censures of excommunication and suspension *ab ingressu*.

Houlbrooke's analysis of matrimonial causes adds little to R. H. Helmholz's magisterial work, *Marriage Litigation in Medieval England* (1974), beyond showing that the "church's pressure" in favor of the public solemnization of matrimony succeeded in curtailing the number of matrimonial cases before the courts. Similarly, the increasing use of the courts for tithe cases is well attested as is their comparative efficiency in testamentary matters. In matters of clerical discipline Houlbrooke can add little to the work of others, particularly in determining whether the courts faced a majority or minority protest against reform (pp. 256–60).

Houlbrooke's statistical tables are invaluable and will provide useful material for comparison. Much of his narrative follows familiar paths, and it is regrettable that he quotes so little from his sources. Repetitive though much of the ecclesiastical court material undoubtedly is, it is punctuated by vivid depositions that bring village life in Tudor England right into the twentieth century. This he suggests in the story of the midwives who were determined to find out the father of an illegitimate child and "examyned (the mother) in moste labor and travaylle" (p. 77); childbirth was hazardous enough anyway, but in the presence of fourteen allegedly honest and

inquisitorial women, it made the rack a positive bed of ease!

This is a workmanlike book that brings together much material. It has come out too late to be pioneering, and one suspects that in his efforts to cut it, Houlbrooke lost much of the distinctive flavor of his evidence.

MARGARET BOWKER  
University of Lancaster

ROBERT ASHTON. *The City and the Court, 1603–1643*. New York: Cambridge University Press. 1979. Pp. xiv, 247. \$24.50.

"The City," like "Wall Street," may be used to indicate a way of life, rather than a geographical location, and it is so used in this book. This book is not in the first instance a study of urban government, nor of the majority of the population of the City of London: it is a study of the world Robert Ashton has now made his own, that of business and politics under the early Stuarts. Its most significant contributions are on the relations of the crown with prominent members of the business community, many of whom also happened to be aldermen of the City of London.

For nearly twenty years, our view of the City of London, and of its leading citizens, has been dominated by the work of Valerie Pearl, whose conclusions are now significantly modified but not superseded. She had stressed the community of interest between the crown and leading members of the business community, those who could be regarded as a "concessionary interest." In terms of the debates conducted in 1961, this was a discovery significant enough to deserve the stress it received. Yet a "concessionary interest," by its very definition, can be fragmented by the making of too many divergent concessions. Ashton has now changed the emphasis by stressing the extent to which, under Charles I but not under James I, the crown dissipated this fund of support, thereby incidentally lessening the tension between privileged companies and the House of Commons. The Commons' vote of thanks to the Merchant Adventurers in December 1641 may serve for an epitome of this change.

Why did this change take place? Ashton is inclined to blame Charles I for his "prodigality" in dissipating business support. Yet this is not a sufficient explanation. Charles I was doubtless a fool, but if it were unnecessary to understand fools, we would all write very short books. Were there any pressures leading Charles to behave as he did? The need for money, and the related need for patronage, led to many difficulties, particularly with the East India Company. Charles did not create these needs, however he may have mishandled them. An ex-

planation of the troubles in the French trade in 1626 should mention Richelieu, as well as the duke of Buckingham. The use of newly incorporated companies to enforce standards of manufacturing, in the cases of the soapboilers and of the bricklayers and tilers, owes something to the lack of a satisfactory method of enforcement, as well as to the folly of the crown. Did the crown possess a satisfactory method for the government of commerce, or was "the concessionary interest" always likely to break up into concessionary interests? This is a valuable book, and it would be nice to see Ashton pursue some of its themes a little further.

CONRAD RUSSELL  
Yale University

STEPHEN D. WHITE. *Sir Edward Coke and "The Grievances of the Commonwealth," 1621-1628*. (Studies in Legal History.) Chapel Hill: University of North Carolina Press. 1979. Pp. xv, 327. \$20.00.

Sir Edward Coke's fame rests not only on his monumental contribution to the development of the common law but also on the important role he played in English parliamentary history, especially in the 1620s. Because of his extensive experience in government, his broad knowledge, and his skill as a speaker, Coke assumed a prominent and influential position in the parliaments of 1621, 1624, 1625, and 1628. Stephen D. White's book explores in great detail this later phase of Coke's parliamentary career. Exploiting the large number of diaries and other parliamentary accounts that have survived from this period, White presents a generally sympathetic treatment of Coke that emphasizes his commitment to reform and denies that he acted as a spokesman for any particular region, interest group, or social class. Even more important, he demonstrates that Coke's political outlook changed significantly during the decade.

The first section of this persuasively argued study deals with Coke's activities in the parliaments of 1621 and 1624. In these assemblies Coke sought mainly to remedy specific grievances, which White treats under the general headings of law, trade, and the privileges and powers of Parliament. The discussion of law is especially valuable in determining exactly what legal changes Coke, who is often depicted as an inflexible legal conservative, desired to bring about. A chapter on economic grievances analyzes Coke's support for the principle of "free trade," which for him was by no means incompatible with governmental regulation of the economy. As far as Parliament itself was concerned, Coke helped to revive the judicial powers of that institution without causing a confrontation with the king. A clash did occur over the issue of free speech late in 1621, when Coke not only supported but

also influenced the wording of the Protestation. In 1624, however, Coke gave ample evidence that he still regarded Parliament as a means of eliminating particular abuses, not as "a forum for the debate of constitutional issues" (p. 179).

In the second section White argues that Coke's concern for remedying specific grievances was transformed during the parliaments of 1625 and 1628 into a more general criticism of the royal court. He traces the emergence of Coke's increasingly ideological approach to political problems, showing how his changed perception of the sources of the country's maladies led him to formulate a more abstract critique of royal power and to insist upon a recognition of fundamental law in the Petition of Right. This shift in Coke's views, the reasons for which the book does not fully explore, resulted in the virtual abandonment of his earlier efforts at reform but made possible the creation of the "rhetorical and constitutional framework" for later attacks on royal government.

White's views regarding the development and the significance of constitutional conflict in the parliaments of the 1620s set him at odds with those revisionists who have stressed the persistence of consensus politics before the Civil War and have minimized the connections between the parliaments of the 1620s and the events of the 1640s. This carefully reasoned study does not by any means, however, simply endorse the conventional interpretation that the revisionists have rejected. White refuses, for example, to identify Coke as the leader of some sort of "opposition party" in the last two parliaments of James I's reign, and at no point does he try to force Coke into a radical mold that clearly he will not fit. But he does give him credit for articulating a set of powerful and durable ideas that eventually served revolutionary purposes. By studying the formulation of these ideas within their proper political context, White has not only shed much valuable light on the operation of Coke's mind but has also deepened our understanding of the long-term causes of the English revolution.

BRIAN P. LEVACK  
University of Texas,  
Austin

MARK A. KISHLANSKY. *The Rise of the New Model Army*. New York: Cambridge University Press. 1979. Pp. xv, 377. \$17.95.

The thrust of Mark A. Kishlansky's argument is unmistakably revisionist. He denies the traditional beliefs that the New Model Army was radical both from and in its conception and that the House of Commons was divided early in the Civil War into conflicting ideological groups or parties. Polariza-

tion took place much later than we have thought: in the Commons, only with the end of the Civil War; in the army, only in the late spring of 1647. Until then, unanimity and the traditional forms were the chief goals of most men.

Kishlansky's estimate of the concern for the preservation of unity and due forms is an important corrective to the tendency of historians to think loosely of opposition and conflict as an integral part of politics. Instead, he shows that divisions and votes were seen as improper. This insight leads to a major modification to accounts of the political career of the New Model and to the suggestion that the crucial turning point was the Commons' attack in March 1647 on the army's right to petition, at a time when the tactics of the Denzil Holles clique in the Derby House Committee were themselves violations of parliamentary norms. The denial of this right added a dangerous new element to the discontents of the soldiery, which until then had been purely professional.

Predictably, analysis of procedure does not always make easy reading, and matters are not helped by occasionally poor proofreading, dangling clauses, an annoying use of jargon, and some very Teutonic compound nouns. More frustrating is the arrangement of the work, with chapters divided into sections on events in the Lords, in the Commons, and in the army. The separate focus on the Lords is novel and highly suggestive, but it is unfortunate that the method involves repetition and sometimes even contradiction. Thus, while Kishlansky argues at length that the hard-pressed parliament under the pragmatic Holles strove in the spring of 1647 to meet the army's material grievances, when he comes to discuss the army's position he reverts to an older view of a hostile parliamentary leadership and contends that it quickly voted concessions in mid-May, once disbandment had been decided, "in a parody" of its earlier delays (p. 218).

As that lapse suggests, Kishlansky is not always able to sustain his revisionism. On page 190 he maintains that the abortive soldiers' petition of March, so critical to his argument, was purely professional; yet on page 201 he suggests that in the early stages there may have been demands for equity and justice at law and complaints against malignants in power, which puts it a little closer to the famous March petition of the Levellers. There are further contradictions, as on the origins of the Manchester-Cromwell quarrel, the significance of frequent divisions, or the extraparliamentary contacts of the "presbyterian" leadership in early 1646. Equally damaging is the basing of his account of "normal" prewar parliamentary practice on pious procedural guides, which results in an alarmingly skewed comparison.

Perhaps a more serious obstacle to persuasion is the lack of adequate reference to the work of other historians. His central claim that the New Model Army was not at first bent on seizing power for radical ends should in justice have contained more reference to the recent work of John Morrill and Murray Tolmie. Those who have identified war, peace, and middle groups, or presbyterians and independents, in parliament cannot be refuted with the mere assertion that the concern for consensus prevailed. They need to be confronted. Consequently, we may wonder, in view of the very striking pattern of resignations of "moderate and presbyterian officers" in the summer of 1647—one field commander per regiment, which Kishlansky finds hard to explain (pp. 182, 219–21)—whether late-1644 politics was partisan after all. For what was the squabble in Manchester's command about? Kishlansky's argument that the perversion of parliamentary procedure undermined institutional legitimacy is extremely important, as is his suggestion about the consistency of the army's later politics. Yet too often he sees procedure as the first cause in political change, rather than as a means. It may be true that Holles's methods were more objectionable than his goals, but when his ally William Strode was reported to have declared early in 1647, "We will destroy them all for Sir Thomas Fairfax will be deceived," then perhaps the army was right to smell a conspiracy.

DEREK HIRST  
Washington University

AMOS C. MILLER. *Sir Richard Grenville of the Civil War*. London: Phillimore, with Rowman and Littlefield, Totowa, N.J. 1979. Pp. 215. \$15.00.

The malevolence of Sir Richard Grenville is a recurring theme in Clarendon's *History of the Rebellion*, and there can be little doubt that the differences between the two that emerged during the Civil War induced a prejudice that colored Clarendon's account. Grenville was fiercely but unconvincingly defended by Lord Lansdowne (1732), but a second kinsman, Roger Granville, wrote a more balanced account (1908). The present biography, which makes good use of manuscript sources, goes far beyond either of these and is undoubtedly definitive.

Grenville came to public attention as a result of his marital troubles. Quarrels over his wife's property led to complex litigation and indirectly to Grenville's slandering her protector, the Earl of Suffolk, with the result that he incurred a harsh sentence in the Star Chamber. Grenville's actions during this phase of his life clearly exemplified both avarice and vindictiveness. Miller's handling of the subject is useful not only in relation to Grenville but

also as illustrating the perils of property litigation at the time.

Grenville fought under Ormonde in Ireland in 1642-43, where his reputation for ferocity and cruelty against both rebels and civilians was noteworthy even in that bloodthirsty arena. Four chapters treat Grenville's part in the western campaigns in 1644-45, culminating in his imprisonment by Prince Charles for refusing to serve under Hopton. This part of his career was most critically related by Clarendon. Miller's account is first-rate. His descriptions of military operations are clear and his analysis and interpretation very convincing. Grenville is shown to have had qualities of leadership, and his soldiers, especially the Cornish, were devoted to him. At the same time he was guilty of the utmost tyranny over the civilian populace, especially the local authorities, and he was very insubordinate and occasionally negligent of orders. Miller justly observes that part of the trouble between Grenville and other commanders arose from faulty royalist administration and from the issuance of conflicting commissions. Here and elsewhere he provides a corrective to some of Clarendon's statements. On the whole, however, Clarendon's account stands up very well, and his general verdict on Grenville still seems valid.

In exile from 1646 until his death in 1659, Grenville nursed his grudge against his enemies at court and in 1653 informed the king that Hyde (the future Clarendon) was Cromwell's spy. In his account of these absurd charges and in his concluding summary, Miller presents a very balanced judgment on Grenville. This book constitutes a welcome addition to the literature of the period.

P. H. HARDACRE  
Vanderbilt University

WILLIAM M. LAMONT. *Richard Baxter and the Millennium: Protestant Imperialism and the English Revolution*. Totowa, N.J.: Rowman and Littlefield or Croom Helm, London. 1979. Pp. 342. \$28.50.

This volume continues the studies of seventeenth-century politics and religion that William M. Lamont began with *Marginal Prynne* (1963) and *Godly Rule* (1969). In the latter book, Lamont wrote that Baxter was never much interested in millennialism and that millennial thought died out after 1660. He has now changed his mind about that: he reads Baxter's *Holy Commonwealth* as a "millennial document" (p. 24) and claims that Baxter based his ideas of Church and state on his interpretation of the Apocalypse. Following John Foxe, Baxter thought the millennium described in *Revelation* was the happy period of the Christian empire under Constantine and his successors. From this, several important conclusions followed: the papacy of this

period could not be *the* antichrist, a statement that shocked and confused Baxter's contemporaries; Commonwealth radicals who looked for the millennium in the future were dangerously deluded; once the papacy became totally corrupt, the correct policy for faithful Christians was to support reformed national churches under Christian rulers like Queen Elizabeth. Charles I was such a ruler until he became the dupe and pawn of popery. Richard Cromwell could have become such a ruler but was brought down by radicals who were probably the pawns of Jesuits. Baxter hoped for a time that Charles II might be such a king. James was a problem for which Baxter earnestly sought the solution by rereading *Revelation*. Then William and Mary gave promise that the modest New Jerusalem that Baxter found in the Apocalypse might appear once more on earth.

Lamont draws startling and original conclusions from his study of Baxter's books and particularly of the hitherto unused manuscripts that record Baxter's study of *Revelation* during his imprisonment in 1685-86. The enemy for Baxter was Catholicism, whose spies, undercover agents, and fellow travelers he discovered everywhere—he thought Harrington's *Oceana* was a popish treatise. Constitutional issues, as Conrad Russell has maintained, were of small importance: Baxter, "like many Protestant Englishmen . . . was more concerned about Popery than the Petition of Right" (p. 25). Pinning his hopes on national churches under Christian rulers, Baxter was bound to oppose separatism in religion and disobedience in politics. He was a conservative who, far from heralding the "spirit of capitalism," wanted powerful rulers who would enforce social justice. "Baxter was nearer to the Welfare State than he was to the acquisitive society" (p. 316).

Lamont's study has the great virtue of putting Baxter into context and making intelligible his paranoia about popery, his interest in witchcraft, and his shifting opinions in religion and politics. It nonetheless raises a few questions. Lamont stresses, for example, that the Apocalypse was the source of Baxter's beliefs, but he does not cite chapter and verse. Where in *Revelation* did Baxter find anything about national churches under Christian rulers? Is the Apocalypse the source of Baxter's ideas or is it the source of the metaphors and symbols in which he clothed his hopes and proposals for reform? How could it be that Winstanley, for example, drew such contrary conclusions from reading the same Apocalypse?

RICHARD SCHLATTER  
Rutgers University

LIONEL K. J. GLASSEY. *Politics and the Appointment of Justices of the Peace, 1675-1720*. (Oxford Historical



Monographs.) New York: Oxford University Press. 1979. Pp. viii, 334. \$29.00.

Lionel K. J. Glassey has chosen to investigate the political use of the royal prerogative to appoint and dismiss justices of the peace. He begins his story with the earl of Danby, who for partisan reasons purged three county benches. Upon Danby's fall, Shaftesbury and his friends sought to build up a country interest in the counties, but Charles II personally blocked their efforts and finally dismissed them from the council. As a result it was the court that remodeled the county benches between 1680 and 1685. By 1685 the Tories controlled the local magistracy, but these Tory squires refused to serve James II's purposes. In 1687 James therefore dismissed 245 justices and named 569 new ones, most of them Catholic. In 1688 he brought Dissenters as well as Catholics into the commissions of the peace. By 1688 he had purged three-quarters of the justices sitting in 1685. As Glassey rightly observes, these massive changes played a significant role in provoking the Glorious Revolution.

In the years following the revolution William III's trimming policies caused the nomination of justices of the peace to fall back into the hands of lords-lieutenants, *custodes rotulorum*, and local magnates. But the turn toward the Whigs from 1693 to 1696 saw political partisanship once again shape the commissions of the peace. Lord Somers, tentatively at first, then more systematically, added Whigs and removed the more extreme Tories. When the Tories gained control of the Great Seal in 1700, they reversed these changes. By 1704 they had created a Tory ascendancy in the countryside, an ascendancy that the Whigs undermined between 1705 and 1710. During these years Lord Cowper removed 195 justices and named 1,044 new ones, creating a Whig preponderance on the county benches. This preponderance was short-lived. Between 1710 and 1714 the Tories removed 405 justices and named 1,725 new ones, mostly Tories. The accession of George I caused the pendulum to swing back to the Whigs, who by 1720 had removed a high proportion of those Tory squires added between 1710 and 1714 and had added enough new justices to give the Whigs a majority at Quarter Sessions.

This bald summary of Glassey's argument gives little idea of its richness, complexity, subtlety, and caution nor of the vast and careful research that supports it. Glassey sees, for example, that most chancellors sought a preponderance, not a monopoly, for their party on the county benches and that they were often dependent on local advice for candidates. He also sees that manipulating the commissions of the peace, despite what many historians have written, had little effect on the general elections fought during these years. Nor did the fre-

quent oscillations in the commissions of the peace have much effect on the actual administration of the counties, since many of the new justices never acted. What the remodeling of the county benches did accomplish was to bind each party together by offering the spoils of office to its local adherents.

Glassey has not written an exciting work—the prose lacks boldness and the argument occasionally gets lost in a welter of detail. But he has written a solid work, whose scholarship is unassailable and whose conclusions are original, perceptive, judicious, and correct.

CLAYTON ROBERTS  
Ohio State University

HERMANN WELLENREUTHER. *Repräsentation und Grundbesitz in England, 1730–1770*. Stuttgart: Klett-Cotta. 1979. Pp. 417. DM 98.

With this detailed work, fated to be controversial, Hermann Wellenreuther presents a revisionist model for understanding the electoral sociology of Britain in the decades before Wilkes and the American Revolution. The author examines the electoral activities of the Duke of Bedford in Tavistock, Bedford borough, Okehampton, Newport, Launceston, Camelford, and Bedfordshire, and the similar activities of Earl Gower in Newcastle-under-Lyme and Lichfield. He challenges the conclusions of J. H. Plumb, John Cannon, and W. A. Speck, contending that these scholars have fundamentally misunderstood the core meaning of the act of voting. He focuses attention on several practices of the era that have in general been neglected by historians: the manipulation of leases to increase the number of voters (for he suggests that the electorate was increasing not decreasing in size!) and the recourse to long-term rather than short-term credit arrangements as a means of fostering enduring ties between landlords and tenants. He argues that scholarly inattention to the power structures in the constituencies and to such local institutions as vestries and corporations—institutions usually empowered to create electorates—has badly skewed our understanding of the era. But behind all these new interests and his iconoclasm Wellenreuther holds fast to an old-fashioned idea as the central truth of Georgian politics: that the relationships between landed proprietors and tenants, far from being exploitative, were reciprocally beneficial—and *seen to be so*.

Wellenreuther applies his conclusions only to the nonurban constituencies. Indeed, one of the major implications of his work is to suggest that the difference between the large urban constituencies and the various smaller or rural constituencies was far greater than we have appreciated. It lay not merely



in the greater complexity of the former but also—even primarily—in the divergent consciousnesses that underlay the choosing of M.P.s in the two types of constituencies. In the large urban ones voting was a straightforward political act, controlled by the relatively sophisticated political perceptions of the electorate, heavily influenced by partisan identifications, and with debate about the candidates turning (at least ostensibly) upon public issues. (Nicholas Rogers, "Aristocratic Clientage, Trade and Independency: Popular Politics in Pre-Radical Westminster," *Past and Present*, 61 [1975]: 70–106, analyzes such a constituency for the 1740s.) But in the rural constituencies—and they constituted a large majority of the total—the choosing of M.P.s was not at all what we of the twentieth century usually take it to be. It was an action occurring in a quasi-feudal matrix, by which electors—tellingly called *Pächter/Wähler* by the author—were in fact choosing not M.P.s but patrons. Indeed, elections were not in the last analysis political acts at all. Instead they were occasions for patrons and electors to confirm their interdependence. The identity of the candidate who happened briefly to stand at the center of this occasion on polling day was of surprisingly little moment. If the patron were adequately meeting the needs of his dependents, the latter would vote for whomever he wished to present. Such considerations lead Wellenreuther to a striking conclusion: a contested election in such a constituency, rather than being a sign of political health, was actually perceived as an indication that the system was functioning imperfectly.

The case is of course unproven. Wellenreuther has damaged his own credibility by making irrelevantly erroneous statements. He has, moreover, examined the electoral activities of only one man (for the comparisons with Gower are too fragmentary to be significant), and in the absence of similar studies of others we cannot know how representative Bedford was nor how persuasively Wellenreuther's interpretive framework will give order to data from other constituencies. But the status of "not proved" should be an incentive to research, not grounds for rejecting the novel conclusions. And such research, the author reminds us, must rely upon the use of largely unquarried sources. Because agents and stewards were the key operatives for any electorally minded landed proprietor, the most important documents for understanding the politics of the era are not the widely used collections of political correspondence but the relatively neglected estate papers and records.

Bubb Dodington once remarked of Georgian politics that "service is obligation, obligation implies return." Wellenreuther argues for the validity of Dodington's judgment. But he does much more. What he gives us is no less than what he believes to

be the true structure of politics at the accession of George III. That familiar claim alone should indicate that, however severe the reservations one may harbor about Wellenreuther's views, this is a work that no student of the era can responsibly ignore.

REED BROWNING  
*Kenyon College*

CHARLES DANIEL SMITH. *The Early Career of Lord North the Prime Minister*. Foreword by the EARL OF GUILFORD. Rutherford, N.J.: Fairleigh Dickinson University Press. 1979. Pp. 335. \$18.00.

Charles Daniel Smith has written a book that is difficult to define. It is not so much a biographical treatment of the early career of Lord North as it is an anecdotal and meandering tour of mid-eighteenth-century politics, somewhat and irregularly limited by the fortunes of its central figure. The first chapter purports to offer evidence that might bolster the old rumor that George III and North had a common paternity, but I find no evidence presented that is both pertinent and new, or at all convincing; and the chapter is permitted to wander off aimlessly into the sexual comportment of the Prince of Wales in a manner that is quite irrelevant, establishing a reputation for Frederick that is hardly in dispute.

There is disappointingly little in the remaining chapters on North's rhetorical style, which is surprising in a work by a professor of speech; and very little indeed on his management of the House of Commons, despite the claims of the preface. The book is clearly a labor of love and has occupied the attention of Smith for over twenty years. Unfortunately, the more important judgments are already to be found in recently published studies by John Cannon, John Brooke, and Peter Thomas. The shorter but fuller and more judicious biography by Thomas provides especially difficult competition, and it is questionable whether anything purely biographical needs to be done on North other than the full-dress biography that Valentine, because of his errors, failed to provide satisfactorily.

DONALD E. GINTER  
*Concordia University*

TODD M. ENDELMAN. *The Jews of Georgian England, 1714–1830: Tradition and Change in a Liberal Society*. Philadelphia: Jewish Publication Society of America. 1979. Pp. xiv, 370. \$14.50.

It is not antisemitic to observe that Jews not infrequently overegg the pudding, and that in the laudable attempt to write their own history they have certainly done so. Todd M. Endelman's book,

however, faces little competition, so shamefully have English writers neglected its subject; a third of the space is given up to Gentiles, so that he contributes to English as well as Jewish history; and he writes modestly and well. Every reader will learn something new about the statistics, setting, or mores of Jewish life in the eighteenth century and, as the author attempts, however briefly, to compare the Jewish experience in England with that abroad, especially in Germany, and with that of other religious minorities in England, there is a context to his discussion.

This is certainly a good book, and if it leaves some sense of disappointment, the reasons are twofold. Though Endelman brandishes some very large and unusable concepts—desacralization, secularization, modernization—his theme is at bottom a simple one, that of the assimilation of the Jewish minority and the consequent threat to its religion and cohesion. This was very obvious in the Jewish upper crust, operating in a world excessively but not exclusively devoted to the worship of mammon. Sampson Gideons were indispensable to English governments, but genteel English society was loathe to accept the sons of Sampson Gideons, however wealthy, without baptism. But the problem affected every social layer of Jewish life. The author gives the impression that English society was exclusive (though depending little on anti-Jewish legislation) and that that tempted Jews into it (he will not have any Herbergian generational hypothesis) was the desire to make more money or to receive the social adulation usually accorded to wealth. Some more precise analysis than this is now required by historians very conscious of how unassimilable the Irish minority has been (especially when compared in Scotland, for example, with the almost total assimilability of the English minority) and by social scientists lamenting the failure of English capitalism to wean the working class from its independent mores.

English society seems, relatively speaking, not to have done badly by the Jews; certainly Jewish emancipation was not followed as it was in Germany, and as Catholic Emancipation was in England, by mob violence designed to keep its beneficiaries in a position of second-class subjects. On the other hand, reasonable Jews, like reasonable Protestant dissenters, could not but be attracted by the cultural advantages that assimilation offered. What did Judaism do to enable its core concepts and practices to be harmonized with those Gentile cultural achievements that were in themselves estimable? Endelman does not say, insisting simply that there was no English Moses Mendelssohn and that Anglo-Jewry was interested in little except for making money in unintellectual ways. He gives no hint of the devices that ultimately enabled English

Jews to achieve high thinking without plain living. Jewish capacity for assimilation is as much a part of English history as English capability to assimilate is a part of Jewish history.

W. R. WARD  
*University of Durham*

F. B. SMITH. *The People's Health, 1830–1910*. (Croom Helm Social History Series.) London: Croom Helm. 1979. Pp. 436. £14.95.

This is an ambitious attempt to describe the whole course of the major diseases and their treatment in Britain over eight crucial decades. It is not a traditional history of the great medical discoveries but of the actual application of medicine to society, with all the complications produced by class assumptions and poverty. The “people” here are chiefly the working classes; provision for the upper classes is mentioned occasionally but mainly for comparison.

The arrangement of the book will cause some irritation. F. B. Smith has decided to take his patients from the cradle to the grave and to relate each disease to the age group it most affected. He begins with childbirth, but the ailments of humanity thereafter refuse to be allocated to particular ages. By the last chapter the format has broken down, and “old age” includes arbitrary sections on influenza, heart disease, quack medicines, and the Poor Law. In fact, there is little organization, for subjects like “hospitals,” “eyesight,” and “school health” could be arranged in any order to equal effect. As in many works of social history, a distinct chronology is also lacking; in covering such a wide range of subjects Smith leaps back and forth between decades, and it is not always easy to locate him without reference to the footnotes. Morbidity statistics impose chronology in the earlier sections, but general subjects like patent medicines are much more rambling.

Smith has another problem common to social historians. He has used mainly printed sources, and the bulk of his material is from the medical journals, especially the *Lancet*. He reproduces many local and regional statistics from these sources, but it is difficult to build up a coherent picture from the diffuse figures for measles in Coventry, smallpox in Birmingham, and so forth. It would be wrong to cavil much at this. The statistics are often from periods when national surveys were erratic and diagnostic methods variable. Smith's information is rich and interesting, but “Britain” appears neither as a unit nor as a series of distinct localities. Readers will wish to trace the numerous references to specific places, and the book demands a place index; for such a substantial work the three-page general index is grossly inadequate.

Smith does not seriously question the value of the medical journals as a source of information on treatment. Their anecdotal pattern is reflected in the book. A section on the *Lancet's* editorial policies would have helped to define the problem; in the early days of the medical journals there was little editorial discrimination between serious research and the lunatic fringe. How then can a particular method of treatment be seen as "typical?"

These defects do not detract from the interest of this book; it is a valuable contribution to a new subject and is fascinating if lugubrious reading. No student of medical history will be able to dispense with it. We trust that Smith has escaped from eighty years of medical literature without a major attack of hypochondria.

M. A. CROWTHER  
University of Glasgow

EDWARD W. ELLSWORTH. *Liberators of the Female Mind: The Shirreff Sisters, Educational Reform, and the Women's Movement*. (Contributions in Women's Studies, number 7.) Westport, Conn.: Greenwood Press. 1979. Pp. xii, 345. \$25.00.

Edward W. Ellsworth's study of the Shirreff sisters is a welcome contribution to the history of education. Emily Shirreff (1814–97) and her younger sister Maria Shirreff Grey (1816–1906) helped systematize women's education in nineteenth-century England. Emily is best known for her writings. Her book *Intellectual Education and its Influence on the Character and Happiness of Women* (1858) gave an impetus to the development of liberal arts curricula in girls' schools, and her pamphlets of the 1870s helped popularize Froebel's method of early childhood education. Maria, more outgoing than her sister, gave public speeches on the need for systematic schooling and used her social contacts to gain support for the Women's Education Union, the Girls' Public Day School Company, and a training and registration society to provide qualified teachers for the company's schools, all of which she organized. Both Shirreff sisters were at the center of discussions of women's issues, as preservers, not revolutionaries. Daughters of a family with relatives among the aristocracy, they reformed middle-class women's education to meet the needs of Victorian society.

Ellsworth discusses the Shirreffs' ideas only in relation to women's education, yet they fitted a broader context. In common with reformers of men's education, the Shirreffs believed that education should take the *form* of rational schooling (defined by systematic learning, regular attendance, and a sequenced curriculum) and have for its *content* the liberal arts (including the sciences and modern languages). The Shirreffs' contribution was to apply

these ideas in theory and in practice to women's education. Until the time that they did so, education for upper- and middle-class girls was unsystematic and vocational. Their families considered that every girl's vocation was, first, to obtain a husband and then to run the household and care for her family. Hence girls were taught sufficient knowledge to be pleasant company for men and to be efficient housewives. By 1880, largely because of the work of Emily Shirreff and Maria Grey, this kind of education had been superseded by rational schooling in the liberal arts on the model of boys' schools.

Ellsworth's study gives a rich description of the Shirreffs' work, placing it within the context of the nineteenth-century women's movement and explaining the influence of their ideas. Despite its strengths, however, the book is at times frustrating. Ellsworth has a disconcerting habit of shifting perspective, like a photographer taking random close-ups of a crowd. His first reference to a woman rarely tells the reader her importance for the topic; rather he describes her relationship to the "significant" men in her life. Lastly, Ellsworth's is a whig interpretation of women's history in which, during the march toward equality, the Shirreff sisters were the liberators of the female mind.

I do not share Ellsworth's view. I suggest that the legacy of the Shirreffs was a mixed blessing to women. Emily, in her writings on early childhood, helped persuade the public that mothers were indispensable companions to their young children, thus encouraging social action against the employment of married women. Similarly, the Shirreffs' emphasis on education in the liberal arts and the need for women to enjoy learning for its own sake was used by some people to counter demands that women be trained for careers. In retrospect, the Shirreffs did as much to make women of the middle classes content with their lot as to break the bounds of existing gender roles.

JOAN N. BURSTYN  
Douglass College,  
Rutgers University

HOWARD L. MALCHOW. *Population Pressures: Emigration and Government in Late Nineteenth-Century Britain*. Palo Alto, Calif.: Society for the Promotion of Science and Scholarship. 1979. Pp. xi, 323. \$18.00.

This work is not a study of emigration per se but a study of the politics of state-assisted emigration during the second half of the nineteenth century in Britain. It traces the origins, development, and demise of the National Association for State Colonization, a pressure group that unsuccessfully lobbied to secure state-assisted emigration. The author also de-

scribes the association's relationship to other diverse groups that favored either state or private aid to emigrants from the 1850s, a period of class cooperation, to the 1890s. The movement reached its zenith during the economically troubled mid-eighties, as part of that decade's rejection of *laissez faire* in favor of collectivism, but characteristically withered away with the return of prosperity.

Howard L. Malchow admirably succeeds in identifying the reasons advanced by those favoring state-assisted emigration, that is, providing markets abroad, avoiding industrial unrest, bolstering the empire, and reducing the supply of labor, while also presenting the arguments marshaled by its opponents, that is, fear of excess labor by colonial trade unions, fear of it serving as an example of "socialist" legislation, resentment by minorities viewing state emigration as a euphemism for transportation, and wariness by union leaders and capitalists who had second thoughts about losing trade union members and skilled labor. For most of the period, both labor leaders and manufacturers, blinded by the fatuities of Cobdenism, regarded emigration as a panacea; they ignored the fact that mates and operatives expelled through the safety valve might serve not only as a market for the workshop of the world but also as competition that might deepen future trade depressions.

Both the author and his publishers have produced a fine book. Writing or reading "loser's history" can on occasion be dispiriting, and the plates from the *Illustrated London News* do much to add life to the subject. Malchow should be complimented, not only for his thorough use of a wide variety of sources but also for his excellent grasp of peripheral issues. On the debit side of the ledger, although the book sheds light on many new characters on the Victorian scene, it contains few surprises. Two exceptions are the fact that Joe Chamberlain, imperialist and friend of the workingman, opposed state-aided emigration and that at times (1878, 1885-87) the annual attrition from emigration was comparable, in numbers at least, to that of British deaths in World War I. Although Malchow does a fine job discussing the "push" of hard times, he generally slights the implications of the "pull" factor on emigration, which operated quite well in the period prior to 1869 and in the two decades preceeding World War I. This was obviously an important factor that contributed to the failure of the movement, which, in combining the characteristics of both an "interest" and "cause" lobby, theoretically should have been strong enough to achieve its aims.

NORBERT C. SOLDON  
West Chester State College

GEOFFREY K. FRY. *The Growth of Government: The Development of Ideas about the Role of the State and the Machin-*

*ery and Functions of Government in Britain since 1780.* London: Frank Cass; distributed by Biblio Distribution Center, Totowa, N.J. 1979. Pp. x, 295. \$20.00.

Of the numerous contributions to the study of modern British administrative history that have appeared during the last two decades, Geoffrey K. Fry's *Growth of Government* cannot be ranked among the more distinguished. The author claims to examine ideas concerning the role of government in Britain between 1780 and the present and to consider the development of governmental functions and machinery over the same period. What is given in the volume, however, does not answer these purposes.

The central difficulty of Fry's work lies not in its conclusions—indeed a number of these make perfectly good sense. He is doubtless right to assert, for example, that Liberals, Green and Keynes in particular, rather than conservatives or socialists, have been the moving force in shaping the intellectual context within which the expansion of government has occurred. His conclusion that the growth of the state in nineteenth-century Britain did not constitute a "revolution in government" certainly merits attention. The problem is that Fry's conclusions rest on no solid analytical structure and consequently lack the force necessary to carry authority and to command respect.

The first part of Fry's book consists largely of a string of quotations from the classical economists, Green, the Fabians, Marshall, and Keynes. Admittedly, the book is a survey, and one has no right to expect detailed analysis, yet the space available could have been put to better use. Unfortunate gaps in Fry's research are also apparent. In the section on J. S. Mill, for example, no reference is made to Mill's substantial article "Centralization," the contents of which are certainly pertinent to Fry's discussion. The work of Alan MacBriar and Willard Wolfe on the Fabians seems to have been neglected in favor of a number of dated treatments (though MacBriar is listed in the bibliography). Nor does Fry always have his facts right. To cite one instance, he states that Mill served in the India Office, "where a Secretary of State and a Council had replaced the Board and the Company" (p. 120). Mill was an employee of the East India Company, and he retired in 1858 when its operation as an agent of government was terminated.

The second part, which focuses on the machinery of government, is no less disappointing. Attempting to say something about the traditional functions of the state as well as the new responsibilities for economic regulation and social provision, this section of the book is almost exclusively descriptive. The connection between the individual thinkers discussed in part 1 and the machinery described in part 2 is never established. As Fry fails to give a



sense of the historical environment inhabited by his Liberal intellectuals, so he fails to locate the institutional developments of the nineteenth and twentieth centuries within their appropriate political and economic contexts, abstracting them from the circumstances that gave them their significance. The result, one regrets to conclude, is a book of very limited value.

BRUCE L. KINZER  
*J. S. Mill Project,*  
*University of Toronto*

ADOLF M. BIRKE. *Pluralismus und Gewerkschaftsautonomie in England: Entstehungsgeschichte einer politischen Theorie*. Stuttgart: Klett-Cotta. 1978. Pp. 258. DM 58.

An excellent book, though slow to start! Adolf M. Birke's aim, far distant, is to show that the rarefied debates on pluralism and state sovereignty—from Maitland to Laski—were prompted by intensely practical, and very puzzling, developments in English law. Thus it is at ground level, in the trade unions' struggles for status, that he begins. From there, through Scots churches' battles for property, and on through a tangle of decisions judicial and legislative, he works upward. But it is a long climb, through dense thickets; by page 124 (the Osborne Case) one has half-forgotten the scholarly peaks that glisten beyond.

After that judgment in 1909, attention shifts to the theorists. But already the basic questions have emerged: can free associations (unions, friendly societies, or churches) find protection in law without losing essential liberties? Is there a middle ground between nonrecognition (with its advantages of voluntarism and autonomy) and full legal incorporation (with risk of state control)? Trade union experience had, in one century, ranged from sheer illegality, through ambiguous toleration, to near immunity. To trade unionists, the ideal remedy was a kind of no man's land where, without recognized incorporate status, statutory safeguards made them practically unanswerable before the law.

But safety for their interest—or any one group's interest—was not the only issue. There was also the issue of protecting the individual against the group: otherwise, privilege or immunity for the “free” association might imperil rights of the members within it. Thus, freedom clashed with freedom, and group freedom with public policy. Group power, in turn, might clash with state power and with ultimate state responsibility. For all these issues, the pluralist theorists cared, but most especially they cared for another value: the integrity and consistency of the law. Birke further shows how these conflicts raised the question of criteria. Should all things be decided by conformity to past law? What was the place of logic? Must both logic and law yield where

a long record of irrational growth and adaptation—irreconcilable with judges' dogma, but functional—dictated other choices? Churchill, the Webbs, Harold Laski (with Holmes intoning wisdom) tossed the questions about, in a result fascinating if inconclusive. Birke links it all to debates among pluralists in today's Germany. For compelling reasons they seek alternatives to the all-competent state.

PAUL BARTON JOHNSON  
*Roosevelt University*

COLIN HOLMES. *Anti-Semitism in British Society, 1876–1939*. New York: Holmes and Meier. 1979. Pp. viii, 328. \$42.50.

There has never really been a “Jewish problem” in Britain, and antisemitism has never been a major force in British society or politics. Nevertheless, the immigration of Jewish refugees from Russia between 1881 and 1914 and from Germany in the 1930s stimulated a significant undercurrent of hostility to Jews. Colin Holmes provides both a microscopic examination of the extremist fringe of antisemitic groups in Britain in this period and an assessment of the wider impact of antisemitic ideas in British society. He begins with a definition of antisemitism as “hostility towards Jews as such,” but he properly notes that antisemitism very often sheltered behind euphemistic “anti-alien” agitation (“aliens” was in the pre-1914 period a code word for Jews much as “immigrants” in Britain today is understood as denoting not the large numbers of white newcomers but black and brown people—many of them now British born). Nevertheless, while clearly conscious of the hypocrisy of the supposed distinction between “anti-alien” and “anti-Jew” feeling, Holmes is perhaps a shade too kind to Balfour's government in concluding that “in its form, it is more appropriate to categorize the 1905 legislation [restricting immigration to Britain] as anti-alien rather than anti-semitic.”

Antisemitism in Britain, as the author rightly stresses, transcended class and party. There was the working-class antisemitism that produced riots in South Wales in 1911 and in east London and Leeds in 1917; there was also the more genteel clubland prejudice against the cosmopolitan friends of Edward VII and the literate scurrility of “Chesterbelloc” and Eliot. Holmes justifiably concentrates on the history of the right-wing antisemitic propagandists, a bizarre collection of cranks, fanatics, and heroes (all three at once in cases such as Sir Richard Burton). But he does not spare the left, pointing, for example, to the anti-alien immigration motions passed by the Trades Union Congress in the 1890s. Only occasionally does a defensive note intrude in the treatment of left-wing attitudes. Commenting on attacks by Communist front organizations in the



1930s on Jewish landlords, Holmes writes: "Throughout the Fascist offensive the CP never forgot that the wider interests of the working class took precedence over those of ethnic interests." Might one not more plausibly argue that the shameless gyrations of West European Communist parties in this period demonstrated rather the CP's total subordination of the interests of the working class to those of the socialist motherland?

One of the most useful passages in this book deals with the antisemitism of Sir Oswald Mosley and the British Union of Fascists. Only rarely does the author seem a little too ready to accept at face value Mosley's subsequent attempts to exculpate himself from the charge of antisemitism—as when we are informed that "Mosley was concerned to engage in reasoned argument against Jewry." The general treatment of Mosley's antisemitism is, however, notably less indulgent than that of Mosley's recent biographer, Robert Skidelsky. Holmes concludes of Mosley that "his attitudes revealed a hostility towards Jews which was expressed within an ethnocentric and conspiratorial framework." Overall, Holmes argues that there was between 1876 and 1939 a continuous tradition of antisemitism in Britain, and he proves his case effectively in this valuable study of the racist underworld of a declining empire.

BERNARD WASSERSTEIN  
*University of Sheffield*

BERNARD WASSERSTEIN. *Britain and the Jews of Europe, 1939–1945*. New York: Clarendon Press of Oxford University Press, for the Institute of Jewish Affairs, London. 1979. Pp. viii, 389. \$17.95.

Britain occupies a special place among those nations that bore mute witness to the destruction of European Jewry. It administered the mandate in Palestine, the most logical haven for the Jewish victims of Germany's extrusion policy, and the governing principles it represented were so antithetical to those of the New Order in Germany that a humanitarian response was in order. Britain's unwillingness to make such a response by altering its wartime priorities to save precious lives and the obtuseness of its governing class reflected in the failure to recognize the special character of the Final Solution is the subject of this well-researched, superbly crafted account. It serves as a sequel to A. J. Sherman's *Island Refuge* (1973), which took the story to 1939.

What emerges from this study, based on newly available foreign and colonial office documents, is that, with the notable exception of Winston Churchill, British decision makers sought to thwart rescue possibilities, a policy that in effect made Britain an adjunct to the destruction process. The Brit-

ish acted as though Berlin's secret weapon was the strategic "dumping" of masses of Jews. The first shots fired by British combatants in the war were not aimed at German soldiers but Jewish "illegals" trying to enter Palestine. British antirefugee policy did not limit itself to a hard-nosed enforcement of the 1939 White Paper. Virtually every opportunity for mass rescue was thwarted either at the top of the decision-making process or at some middle level. A separate Jewish fighting force was ultimately so pared down that it lost all symbolic significance. Statements of retribution that might have helped in breaking through the curtain of silence surrounding the genocide were not made because it was believed that atrocity propaganda would be counterproductive. Retributive bombing of German cities, twice suggested by the Polish government-in-exile as well as the bombing of the gas chambers and crematoria were rejected as were specific offers by Rumania and Hungary to release thousands of Jews. Even the idea of sending food packages to certain camps was opposed because it flaunted the blockade on which British strategy traditionally placed high hopes. Yet throughout the war occupied Greece was fed by Britain. Taken together these policies made it appear as if British officialdom preferred the Nazis to liquidate the Jews rather than have to assume the burden that rescue entailed. That indeed was what Joseph Goebbels believed.

Bernard Wasserstein observes that the Jewish image as a powerful force that led the British to issue the Balfour Declaration in 1917 was altered considerably during the interwar period. During World War II Jews were considered one of many groups victimized by Nazi barbarity. Their "wailing" for special attention made them a strategic liability, a threat to the principal priority of winning the war as quickly as possible. The British governing establishment was too far removed from the stench of the death camps to perceive that Nazi harnessing of modern mass production techniques to produce death did indeed differentiate the Jewish crucible from other atrocities committed during the war. It could not perceive that such an act of social cannibalism required draconic reprisals or at least a recognition of its significance.

The author's conclusion regarding British leadership seems inescapable. It "experienced an imaginative failure to grasp the full meaning of decisions, when the consequences were distant, unseen, and bore no direct relation to the actor" (p. 356). That conclusion will come as no surprise to those who remember Munich or the scandals that wracked the British establishment in the postwar period.

HENRY L. FEINGOLD  
*Baruch College,  
City University of New York*

S. A. WALKLAND, editor. *The House of Commons in the Twentieth Century: Essays by Members of the Study of Parliament Group*. New York: Clarendon Press of Oxford University Press. 1979. Pp. vi, 649. \$55.00.

*The House of Commons in the Twentieth Century*, edited by S. A. Walkland, is a series of essays by members of the Study of Parliament Group. The genesis (or rationalization) for this book is a three-volume work, published in 1908, by Josef Redlich. Redlich's study of the procedure of the House of Commons is a remarkable work. The Austrian scholar realized, in a way that few English writers or, indeed, perhaps only one other foreigner, the American Lowell, did, that the British clearly had a constitution, albeit not a written one. Redlich was also important for his realization that the real power in the House of Commons, and its contribution to the constitution, was procedure—hence, the importance of Redlich's work and this effort to honor it by bringing out a series of essays that cover many of the topics covered in the earlier study.

There is much in these essays that is both interesting and important. At the same time, one can understand why it was Lowell and Redlich who were the perceptive ones. Just as de Toqueville and Bryce saw American life in some ways more perceptively than their American contemporaries, so too there is something slightly chauvinistic about this new book of essays, excellent as they sometimes are.

The essays, for instance, show all the advantages and disadvantages of the state of political science in England. The English approach to political science is still more historical than the American, although not necessarily the worse for that. The absence of theory, however, and some hesitation about the use of quantitative studies, does at times frustrate the intellect even while the absence of jargon may soothe the nerves. So too, the English are much more willing than Americans, in books of essays such as this, to rely on those who are practitioners of the art rather than on trained political scientists, or even, as they would put it, professors of government. The effect of this is to underline the slightly formalistic approach to political science. Although English political scientists have moved beyond the state of assuming that civil servants are concerned only with technical issues and never with policy issues, and judges only with law and never with policy, one at times wonders how far from that intellectual position some of the contributors to this book in fact have strayed.

There are, however, some excellent essays, combining the best of the elegant English style and mastery of history with the subtle use of the theory and quantitative approaches that dominate American political science. In that category one would have to

put Michael Rush's essay on the "Members of Parliament" and Walkland's own essay on "Government Legislation in the House of Commons." Rush produces a subtle blend of quantitative expertise and the historical tradition in his study of members of Parliament, while Walkland analyzes the specialist committees and their role in legislation in a smooth and elegant way.

Even these outstanding essays, however, underline another weakness in the book. Writing about the House of Commons and basically about procedure in the house, is at times like studying Hamlet without the prince. The current moves in the Labour Party to keep members of Parliament on a very short leash and far more subject both to local party officials and to the national executive are an integral part of comprehending the style of the typical member of Parliament. The projected changes in the specialist committees and the role of the civil service in overtly and covertly opposing their extension are essential for understanding the process of legislation in the house. Yet, the inevitable context of these issues is covered at best, glancingly. Hence, in spite of the elegance and erudition, one, at times, has a strong sense of frustration.

Perhaps the book is at its very best in Marshall's essay on privilege. Geoffrey Marshall is among the outstanding political scientists in England today, although not as well known in this country as he should be. His essay on "The House of Commons and its Privileges" is not only both comparative and perceptive but also opens up many wider notions. The House of Commons has at times been somewhat ludicrous in protecting its so-called privileges; and although there is now a more relaxed approach to parliamentary privilege, Marshall's trenchant criticisms have to be read by anyone concerned with the operation of modern government in England today.

Peter Richards adds a useful short essay on the private members bill. Nevil Johnson has a thoughtful essay on select committees, suggesting the waning of such committees as well as their relationship to royal commissions. Johnson's essay is particularly important on the statutory instrument and the Public Accounts Committee, although it is difficult not to contrast his conclusions with those of Richard Crossman in his *Diaries* (1977). Meanwhile, Philip Norton's essay on the parliamentary parties, with heavy emphasis on the work of the Whips and their offices is an interesting excursion into an almost totally neglected area. The essay, however, would be better still if there were more emphasis on the change over the last decade resulting from a more full-time membership in the House of Commons. Perhaps better integration among these various essays would also have been useful.

As suggested earlier, perhaps the weakest essays

from a scholarly point of view are those by persons who are more practitioners than scholars. Ryle's essay on "Supply and Other Financial Procedures" is exhaustive, while Borthwick's essay on questions and debates falls into the same category, as does Philip Landy's excursus on the speaker and his office. All are encyclopedic but lacking in theory and principle and sadly, on occasions, both in structure and rigorous editing.

It would be wrong, however, to dismiss this book easily. It is essential reading for anyone concerned with the House of Commons and the modern British Constitution. Its defects are perhaps those of books of essays rather than this particular volume. Each essay adds something of substance and must help us to comprehend better the procedure of the house and thus of the British Constitution.

ROBERT STEVENS  
Haverford College

ROY CHURCH. *Herbert Austin: The British Motor Car Industry to 1941*. (Europa Library of Business Biography.) London: Europa Publications. 1979. Pp. lii, 233. £12.00.

This important and well-researched book traces the origins and evolution of one of the U.K.'s main car makers, the influence of its founder Herbert Austin, and the effects of the company on the U.K. car industry. Although Austin's interwar activities gave little hint as to the hectic nature of the industry's postwar history, a book like this can explain much of the car industry's present structure, behavior, and performance.

Despite Herbert Austin's early experiences with Wolseley, which encouraged him to seek the ownership of a business as well as its management, this did not prevent him from falling into the common British trap of designing cars for a limited market. The results were disastrous, and the crisis of 1920-22 was surmounted only by a drastic reorganization and reassessment of company policy. Even then, the financial effects of the crisis were to last until the mid-1930s. Nevertheless, Austin's position recovered, not least because, for a time, head-on competition with Morris was avoided. Roy Church neatly summarizes these issues.

Although concentrating on model policy, Church's limited discussion on pricing policy is useful. For instance, it shows that an attempt at price rigging occurred (p. 116), but this did not prevent the appearance of a cut-price Austin (p. 128) or competitive responses (pp. 128-30). An aspect of model policy seen for its commercial acumen and rapid gestation (pp. 116-17) could also describe a company hesitating until shown the way by a competitor.

What could be a commentary on today's problems is the analysis of the car makers' criticisms of the U.K. steel industry (p. 122) and their attitude to consumer preference (p. 128). References are made to Austin's lagging styles and designs and Morris's superiority (p. 127), but matters should not be overstated: the Austin 10 h.p. engine of 1934 was an advanced design, and time was to show the greater efficiency of Austin's component production.

Notwithstanding its subtitle, this volume is not a completely rounded history of the car industry. For instance, before the mass production of steel bodies the main production technology of the major car firms was machining and metal removing, not sheet metal forming. This technology was supplied by independent bodybuilders, and to omit their role is to miss the economic significance of, say, Rootes buying about 85 percent of a car's ex-works value outside the company: any analysis of this, as on pages 135-36, is disappointingly brief. Again, as Austin made no purpose-designed commercial vehicles between 1925 and 1939, Church does not analyze this sector of the industry, although most of the major car firms were engaged in making commercial vehicles. Indeed, Adderley Park *was* closely integrated within the Morris empire (p. 106), but from 1929 and as part of Morris Commercial.

In a book of such scholarship, lapses such as wrongly cited tables (Table 8, p. 130), a poor matching of text and tables (p. 133), confusing (p. 117, no. 23) or doubtful (p. 186, no. 5) footnotes, oddly titled or omitted references may be irritating, but Church's excellent volume must be regarded as a valuable addition to the literature on the economics of the car industry.

D. G. RHYS  
University College,  
Cardiff

WALTER MAKEY. *The Church of the Covenant, 1637-1651: Revolution and Social Change in Scotland*. Edinburgh: John Donald; distributed by Humanities Press, Atlantic Highlands, N.J. 1979. Pp. vii, 216. \$27.75.

The history of Scotland in the seventeenth century has received its fair share of attention over the past three centuries. Many of the earlier works on the Covenanting movements have produced more heat than light, which is usually the case when religion flies in the face of politics. David Stevenson and Walter Makey, however, have produced scholarly works on the period that have redressed this particular historical deficiency. The former has written two books and numerous articles on the subject, and the latter has produced this interesting book that emphasizes the social change in Scotland before, during, and after the Covenanting movement.

This social perspective is the obvious strength of the work. Where it differs exactly from Stevenson's previous work is more of a mystery.

The book is divided into two parts with a brief introduction called the "Silent Revolution." The "Story of a Revolution" makes up the first and weaker of the two parts. The nonspecialist will have difficulty here because an enormous amount of the history of the period is assumed. The second part, "Anatomy of a Revolution," is well researched and contains many interesting ideas. Makey has been careful not to stretch his research material too far, and he cautiously warns the reader about the strengths and weaknesses of his statistical data. These data, the "stuff" of social historical analysis, provide the necessary material for the study of the origins of the ministers and elders.

There seems to be some confusion about a few important concepts, such as conventicles, privy kirks, and presbytery. Certainly conventicles were similar to privy kirks, but there were essential differences: privy kirks opposed an established church and elected their ministers, while conventicles recognized the established church and did not elect church officials. The presbytery was both a church court and a geographical area; it is unlikely, however, that the church was in the tax-collecting business.

During the "Silent Revolution," according to Makey, the ministers disagreed "about almost everything that was fundamental" (p. 184). On the last page he seems to contradict this: "The differences between the two parties were real enough but they were tactical rather than fundamental" (p. 185). In any case, they seem to have agreed that feudalism in Scotland was definitely on the decline by the end of the seventeenth century.

Makey's publisher is to be commended for making so much recent scholarly work available to the interested scholar and student of Scottish studies. Nevertheless, care must be taken that dissertations are carefully rewritten and that serious printing errors are eliminated. Although Makey revised his thesis for publication, expanded part of it, and divided his "A State Opposite to a State," much of it is unchanged. Nonetheless, the book will add much to our social awareness of the Covenanting movement of the seventeenth century.

CHARLES H. HAWS  
Old Dominion University

IAN WHYTE. *Agriculture and Society in Seventeenth-Century Scotland*. Edinburgh: John Donald; distributed by Humanities Press, Atlantic Highlands, N.J. 1979. Pp. ix, 301. \$35.00.

Ian Whyte has given us a valuable account of Scottish agriculture in the seventeenth century. He

questions the traditional view that agriculture in Scotland remained basically unchanged from the middle of the sixteenth century until the agricultural "revolution" of the eighteenth century. He argues instead that a considerable degree of change took place during the seventeenth century—at least in the Lowlands—with the result that crop yields improved and tenants gained increased security through longer, written leases. This modest agricultural improvement—and Whyte recognizes its slow pace—was not without setbacks, such as that brought by the political turmoil of the 1640s and 1650s and the terrible famine of the later 1690s. But on the whole, Whyte convincingly shows, the century saw moderate agricultural progress, marked by the increased use of fertilizer and lime, better crop rotations, and, in the eastern Lowlands, a shift away from subsistence to commercial agriculture.

This progress, however, did not free Scotland from a terrible subsistence crisis in the 1690s. Bad weather in 1695, 1696, and 1698 drastically reduced the harvest in many parts of the kingdom, and the ensuing famine cruelly cut back the number of small tenants and laborers, particularly along the Borders and in the Highlands. Despite the advances the century had witnessed, Scotland still remained agriculturally primitive, unable to adjust to severe climatic shocks. One wonders what the loss of life in the 1690s would have been had no agricultural improvement taken place earlier.

Throughout the seventeenth century, the smaller tenants and the agricultural laborers subsisted—when they subsisted—on a monotonous diet of oats and a hardy form of barley known as "bere." Notwithstanding the pastoral nature of the agriculture, meat or dairy products seem to have been little eaten by the poorer sorts. But Whyte points out the interesting fact that even the poorest Scots often had kale yards adjoining their cottages. Kale is rich in vitamins A and C, and it provided a certain balance to a diet otherwise too heavily weighted toward cereals.

Although Whyte's knowledge of Scottish agriculture is impressive and the book is obviously the result of painstaking research in a wide variety of estate papers and other primary sources, the sections on demography are rather superficial. Perhaps *Scottish Population History from the 17th century to the 1930s* (1977), edited by Michael Flinn—the standard work on Scottish historical demography—was not yet available when this book went to press, although Whyte cites other works published the same year. Nor does Whyte seem well grounded in studies of subsistence crises or agricultural practices in other climatically marginal areas of Europe, such as Scandinavia, Switzerland, or northwest England. Wider reading would have enabled him to place his Scottish evidence within a broader context. These



minor faults, however, in no way detract from the importance of the material he has gathered on Scottish agriculture.

The book is nicely designed but unfortunately very carelessly printed.

ANDREW B. APPLEBY  
San Diego State University

BRENDAN BRADSHAW. *The Irish Constitutional Revolution of the Sixteenth Century*. New York: Cambridge University Press. 1979. Pp. 303. \$34.50.

In this major reinterpretation of Irish history during the middle Tudor period, Brendan Bradshaw finds reformation and revolution where conventional historians have seen little more than rebellions and reaction.

Bradshaw devotes nearly a third of his book to providing necessary background. He examines the medieval Irish lordship to show the origins of the problems of government that were its legacy to the sixteenth century. He also discusses the failures of Henry VII via Poyning's expedition and Henry VIII via Surrey's expedition to deal with those problems effectually. And he traces the development of a movement for political reform among the Anglo-Irish of the Pale through an examination of their political treatises.

After setting the stage, Bradshaw turns to the middle Tudor period, beginning with the reform of the Irish lordship in the era of Thomas Cromwell. The Cromwellian reform was restricted to the Anglo-Irish areas. There Cromwell ended the long-standing rule by feudal magnates and revived crown government. Crown government, however, did not mean Irish government. In accordance with Cromwell's concept of unitary sovereignty, the real government of Ireland in both church and state resided in England. As for the "disobedient" Irish, Cromwell established a permanent English garrison to restrain them in their state of insecurity while awaiting an indefinitely postponed conquest.

Cromwell's fall was followed by what Bradshaw calls "the liberal revolution." Its principal architects were the English lord deputy, Sir Anthony St. Leger, and his Anglo-Irish cohort, Sir Thomas Cusack. The liberals had two objectives. One was to assimilate the Irish by consent instead of conquest. This was to be accomplished via the process of surrender and regrant whereby the "unconstitutional" Gaelic lords were granted feudal titles and security of tenure in exchange for submission to the jurisdiction of the crown. The other objective was to create an Irish nation. In this interest the liberals induced a reluctant Henry VIII to accept the act changing his title from lord of Ireland to king of Ireland. Ireland became, like England, a realm of the king, a sover-

eign kingdom. Ireland's new status enabled St. Leger to restore Dublin as its center of administration, and he made a small start in transforming the Irish Parliament into a national legislature by including some Gaelic lords. St. Leger saw royal sovereignty as including the royal ecclesiastical supremacy, which he expected all Irishmen to accept, but he allowed the introduction of religious reforms to await persuasion and education. The death of the supportive Henry VIII brought the liberal experiment to an end.

Bradshaw's version of Irish history during the middle Tudor period differs substantially from previous versions. Such a revision is bound to produce controversy, but those who enter the lists will have a formidable array of evidence and argument to overcome. Irish history during the middle Tudor period should be a lively subject for some time to come.

MORTIMER LEVINE  
West Virginia University

OLIVER MACDONAGH. *Ireland: The Union and its Aftermath*. Rev. ed. London: George Allen and Unwin. 1977. Pp. 176.

Many students of Irish history recognize that one of the most incisive minds working in the field is that of Oliver MacDonagh of the Australian National University. Part of the reason for this influence is a short book, entitled simply *Ireland*, that appeared as part of the Modern Nations in Historical Perspective series in 1968. Although *Ireland* has had a significant impact on Irish historians, its publication was virtually unheralded in North America during the late 1960s. With interest in Ireland growing in colleges and universities, it is gratifying to welcome the revised and enlarged second edition of *Ireland: The Union and its Aftermath*. MacDonagh's book is not a history of Ireland since 1800 but rather a discussion and analysis of that history. It is not a book for the reader who does not have some grounding in modern Irish history. But the person who wants a better understanding of Ireland during the last two centuries will find *Ireland* packed with compelling observations, ideas, and, of course, ironies.

The original work examined six principal themes in the development of modern Ireland. These chapters appear with only minor modifications in the second edition, along with a new section of "historical speculation" on the evolution and possible direction of the Northern Ireland crisis. MacDonagh begins with a detailed discussion of the nature and meaning of the Anglo-Irish legislative union of 1801 to establish the setting for the development of his themes. The Union "fenced in the range of the politically possible" (p. 13) for more than a century



and was crucial in molding people and events. But MacDonagh, a student of the emergence of the modern machinery of government, demonstrates that the Union brought not only more direct British control but also the early fruits of the modern and impartial state. In the second chapter we see that, despite Irish resistance to absorption into a truly united kingdom, Ireland, if not purposely a "social laboratory," was, in fact, a proving ground for structures and policies that were tried only later, sometimes generations later, in Britain. Ireland had a paid, professional magistracy, a coherent, national police force, a national system of health dispensaries, and a national school system before Britain. True, the need was more plain and pressing in Ireland, but the government response was positive, which bequeathed to twentieth-century Ireland a seasoned administrative structure and relevant experience in state involvement in social and economic programming.

The subtle modernization of Ireland under the Union is a theme that is also apparent in MacDonagh's discussion of the development of Irish nationalism and the independent Irish state in the years following 1922. Even though the evolution of the Irish national idea was a direct response to British rule and often—particularly in Young Ireland, the Gaelic League, and the Gaelic Athletic Association—expressed itself in terms of a traditional Gaelic and agrarian ideal, the long, intermittent struggle against the Union bore the hallmarks of profound modernity. O'Connell's mass movements, which mobilized hundreds of thousands of Irishmen in support of national political objectives by utilizing the national organization furnished by the Catholic Church, established a durable pattern for future constitutional movements that instilled a consciousness and tactical skill that made Ireland one of the most politically modern countries of the nineteenth century. The successful establishment of the new state in 1922 contains an irony in that the Irish Free State pursued a traditionally British policy with the aid of a substantially British-trained civil service and a very able executive that had learned well the lessons of a century of administrative development. Although the first Free State government forced a determined and armed republican opposition into constitutional politics, MacDonagh includes a good discussion of how the Anglo-Irish settlement was steadily undone not only by De Valera during the 1930s and 1940s but also by the first independent government.

*Ireland* concludes with a valuable new section that traces the Irish "tradition of alternative government" (p. 144) that has contributed to the Northern Ireland crisis. The "Whiteboy" origins of opposition politics in Ireland, which refer to local agrarian groups of the late eighteenth century that organized

to oppose changes perceived as being in any way adverse, are seen in contemporary Ulster politics, as both communities try to establish alternative structures to preserve their respective heritages and interests. The range of our knowledge and understanding of Irish history has increased tremendously since 1968, but *Ireland* holds up well and has a freshness that marks MacDonagh as a hard-thinking and forward-looking scholar. *Ireland* is thoroughly stimulating and enjoyable reading, and it is an excellent selection for graduate students who want to understand better Britain's Irish problems.

W. J. LOWE  
State University of New York,  
College at Cortland

EMMET LARKIN. *The Roman Catholic Church and the Plan of Campaign in Ireland, 1886-1888*. Cork: Cork University Press. 1978. Pp. xv, 334. £9.00.

EMMET LARKIN. *The Roman Catholic Church in Ireland and the Fall of Parnell, 1888-1891*. Chapel Hill: University of North Carolina Press. 1979. Pp. xxi, 316. \$19.00.

These two volumes are serial to Emmet Larkin's 1975 study, *The Roman Catholic Church and the Creation of the Modern Irish State, 1878-1886*. The first develops the theme of the earlier volume for the years 1886-88, the second continues the story for the 1888-91 period, which witnessed the fall of Parnell. Like the earlier work, these two books present, in exhaustive detail, the correspondence of members of the Irish Catholic hierarchy with each other, with Rome, and with members of the government who supported the clerical-nationalist alliance of the Parnell years. Larkin has severely restricted his sampling of letters to those that throw light on the maneuverings of a peculiarly political generation of bishops. Reading rather like extended articles from a learned journal, the chapters of the books are composed of a clever linking together of letters to form a narrative account of the evidence that Larkin fits into a historical model to explain the development of Irish Roman Catholicism at this time. Each volume has a prologue in which the thesis of the work is set out and an epilogue to indicate how the evidence of the work fits into the model.

The two books represent, with Larkin's earlier volume, a remarkable tour de force, with other volumes promised. Historians of Irish affairs will long appreciate Larkin's shrewd assessments of the often devious political maneuverings of calculating prelates, like the nationalist archbishops, William Walsh of Dublin and Thomas Croke of Cashel. Larkin delights in exploring their complex motives of behavior, almost on a day-to-day basis; most of his judgments have a ring of truth about them. His his-

torical empathy allows him to share the antipathy of most nationalist bishops of the "Plan of Campaign" era toward a difficult prelate like Edward Thomas O'Dwyer of Limerick, who refused to march to the Parnell drum. On the other hand, he also notes the thought of priests like Arthur Griffin of Kerry who represented the tradition of the Cardinal Cullen and Bishop Moriarty era by protesting murder and other agrarian outrage.

Scholars, of course, will question the inevitable "sampling error" that arises from Larkin's historical technique, which he labels "mosaic." He selects and arranges manuscript evidence to form "a representation of what was true," as he says in the prologue of both his books. His hope is that by the sheer volume of the evidence he presents, including whole letters, the reader will be persuaded to accept the "reality" of his "portrait." He admits, however, that the evidence is fitted into the "mind's eye" pattern of history held by the author and it will naturally not be appreciated by everyone.

When he speaks, for example, of the "making and consolidating of the *de facto* Irish state" (vol. 1, p. 318), is he not, in his "mind's eye," referring to a southern Irish Catholic confessional state, like that of the present republic, rather than to an "Irish state"? Against his use of a kind of Whig version of history, some historians would argue that there is a not inconsiderable portion of the Irish population, who have never welcomed the kind of "state" that, Larkin argues, Parnell had already brought into existence. His "pattern" seems to suggest that Larkin has identified himself with those nationalist historians who have tried to simplify Irish history by pretending that Ulster and its intransigent population simply do not exist. Quite apart from the Protestant population, which generally had no use for Home Rule, Ulster had some moderate Catholic prelates who hardly fitted easily into the southern Catholic-Nationalist alliance. If Daniel McGettigan of Armagh, for example, had belonged to the tradition of Walsh and Croke, his funeral would not have been graced by the presence of the Protestant primate and droves of Protestant clergy and laity.

Apart from this common error of many Irish historians, denying Ulster its existence by ignoring its history, we are all greatly in debt to Larkin for these two volumes, filled with so much hitherto unpublished material. When historians of different persuasion from Emmet Larkin look at his evidence, some will question his "portrait" of the age, for all "mosaics" seem at times to be lacking in dimension. It is doubtful, however, if any historian will improve much upon his sad picture of how secular and power-hungry the Irish "priest in politics" can become.

DESMOND BOWEN  
*Queen's University,  
Belfast*

PATRICK BUCKLAND. *The Factory of Grievances: Devolved Government in Northern Ireland, 1921-39*. New York: Barnes and Noble, and Gill and Macmillan, Dublin. 1979. Pp. xiii, 364. \$27.50.

In the first work to make extensive use of recently opened cabinet and departmental archives, Patrick Buckland has explored in detail the workings of the Northern Ireland government in the interwar years. The first of three major sections describes and analyzes the overall framework of the provincial regime and its relationship to local authorities and to Westminster. The author then deals with those functions (finance, trade and industry, agriculture, social services) that have not usually been the main focus of Catholic and nationalist criticism of the regime. Finally, he turns to the governmental powers that usually figure most prominently in discussions of Northern Ireland because of allegations of discrimination against the Catholic minority in their exercise: law and order, justice, and the control of electoral and educational arrangements.

Buckland has striven mightily to make interesting reading of such topics as the Joint Exchequer Board and the 1924 Eggs Marketing Act. Readers should resist the temptation to skip over such topics to get to the material that seems to bear more directly on contemporary sectarian conflict. The book's great contribution is to demonstrate, by examining routine government functions, that the central problem of the Northern Ireland regime was not its draconian power but its tragic inefficacy.

Although he favors partition "as the most practical way of reconciling the apprehensions of Ulster Unionism with the aspirations of Irish nationalism" (p. ix), Buckland finds little to praise in the workings of the northern government. Even in its exercise of those functions largely irrelevant to the minority problem, he finds only limited areas—for example, agricultural policy—in which the devolved administration managed to make a positive contribution to its citizens' welfare. He argues that the existence of sectarian polarization in Northern Ireland only exacerbated the fundamental unworkability of devolution within the United Kingdom. The book is therefore a contribution to contemporary discussion of devolved government for Scotland and Wales, as well as to Irish history. "There really is no half-way house," Buckland concludes, "between union and complete separation" (p. 280).

The author's commitment to deal only with topics on which departmental archives were available to him sometimes seems unfortunate. For example, he breaks off his account of "law and order" issues in 1922 because constabulary records are still closed after that date. Although he obviously could not treat that issue definitively for 1923-39, it would have been useful to have at least a brief sketch of what can be known from published sources. Never-

theless, within the limits he sets for himself, Buckland's research is thorough, his analysis keen, and his judgment sound.

DAVID W. MILLER  
Carnegie-Mellon University

JEAN-PIERRE GUTTON. *La sociabilité villageoise dans l'ancienne France: Solidarités et voisinages du XVI<sup>e</sup> au XVIII<sup>e</sup> siècle.* (Le Temps et les Hommes.) Paris: Hachette. 1979. Pp. 294. 65 fr.

For some years there has been a need for a good synthesis of recent important work on village life in Old Regime France. Jean-Pierre Gutton has provided us with that synthesis. His modest-sized book will give the nonspecialist some excellent insights into the *sociabilité* of French peasants.

The book's coverage is more modest than its title at first indicates. It is in fact a summation of village *sociabilité* during the seventeenth and eighteenth centuries with occasional forays into the fifteenth and sixteenth and very rare references to the middle ages. But even that is a staggering undertaking, for what the French mean by *sociabilité* is nothing less than the entire anthropology of village life.

Gutton's book will not provide many new insights for the specialist in Old Regime social history—nor is it so intended. His main thesis is that peasant life had for centuries been centered on village communities that provided a nearly complete universe for their members. In the seventeenth and eighteenth centuries, however, most of these communities were weakened under the dual attack of statism and the Counter Reformation.

Gutton summarizes the social structure of peasant communities and discusses various forms of families to be found (a reality far more complex than the "extended-nuclear" dichotomy so beloved by sociologists), internal polity, and relations with state and church. I found the discussions of family structure, village government, tax collection, the nature of the *seigneurie*, and the changing role of the church especially well done. There is a great deal of noteworthy information given by means of well-chosen examples. Indeed, a good many lectures on European social history will be rewritten after reading this book. To take only one example, Gutton destroys that cherished belief that the typical parish priests were "of the people," in large part promising young peasants who made it up the social ladder. As it turns out, this was rarely the case. In the dioceses of Tarbes, La Rochelle, and Autun in the seventeenth century and that of Strasbourg in the eighteenth, the parish priesthood was mainly of bourgeois or noble origin. In lower Auvergne in the eighteenth century 10 percent were sons of rich *laboureurs* and the remaining 90 percent sons of magistrates, merchants, or well-to-do artisans. For those

not conversant with the research of the past decade, Gutton's book is filled with such revelations. Above all it conveys a sense of the enormous complexity of village *sociabilité* from region to region.

There are two modest criticisms to be made of the book. First, Gutton is uneven in his treatment of the regions of France. His own monographic research has been concentrated in the Lyonnais, and he naturally seizes upon that region for the greatest number of examples. Some regions rarely get mentioned. Second, very little research by non-Frenchmen is cited. Both of these criticisms are minor because to push them would be to demand a different book rather than critique this one. *La sociabilité villageoise* is not the final summation. It is a modest beginning, but one that deserves to be widely read. Indeed, I hope that some publisher sees fits to translate it, for the book would be even more valuable for advanced undergraduates and others who are not comfortable reading in French.

JOHN B. CAMERON, JR.  
University of Southwestern Louisiana

JACQUELINE-LUCIENNE LAFON. *Les Députés du Commerce et l'Ordonnance de Mars 1673: Les juridictions consulaires, principe et compétence.* Preface by JEAN IMBERT. Paris: Editions Cujas. 1979. Pp. ix, 153.

The deputies of commerce represented the interests of trade and industry before the Council of Commerce, created in 1700, and the Bureau of Commerce that supplanted the council and lasted from 1722 until the Revolution. There were twelve deputies, and they were selected from the upper ranks of the merchant aristocracies in the leading commercial cities of eighteenth-century France. Their function was to advise royal *commissaires* in the council or bureau on a variety of economic, financial, and administrative issues affecting French merchants.

Jacqueline-Lucienne Lafon, a student of commercial and public law, examines the role of the deputies in the interpretation and possible reform of Colbert's commercial ordinance of 1673. Her most important primary source is the F<sup>12</sup> series ("commerce et industrie") at the Archives Nationales, which contains a subseries devoted to the Council and Bureau of Commerce. As a first step toward a general study, she has written a short monograph on the relations between the deputies and the "consular" law courts that, though initiated in 1563, looked back to the commercial ordinance as their main charter. The sixty-seven *juridictions consulaires* consisted of part-time judges called "consuls," who were in fact merchants drawn from the most powerful merchant corporations. They had competence over mercantile and business litigation, with the

parlements and the Royal Council sharing appellate jurisdiction.

The main feature of Lafon's study is to show that the deputies actively protected these business courts from the threats of regular tribunals, who wished them abolished, and from the ambitions of lesser merchants in other towns, who hoped that they would proliferate. In these efforts the deputies were successful, and the *juridictions consulaires* of the eighteenth century were essentially those of the late reign of Louis XIV. On the other hand, the deputies failed to extend legal competence beyond the territorial limits of the town or bailliage where the courts sat, and they were rebuked more than once in their efforts to enlarge the definitions of business activity then used for legal purposes.

Although the author's perspectives are rigorously narrow and she is overly generous in her praise of the deputies (who are always diligent, active, well informed, and so on), her legal analyses are most carefully done; and she provides some insights into the actual functioning of the bureaucracy of the Old Regime.

JOHN J. HURT  
University of Delaware

ALAIN LOTTIN. *Chavatte, ouvrier lillois: Un contemporain de Louis XIV*. Paris: Flammarion. 1979. Pp. 445.

Pierre Ignace Chavatte (1633–93) was a modest *sayetteur* (a weaver of a local variety of inexpensive woollens) who lived and worked in the poorest quarter of seventeenth-century Lille. His father was a *maître sayetteur* who had been driven by hard times to hire out his services and who had once been convicted and pardoned for murdering a man in a drunken brawl. His mother and his wife were both midwives, and their work gave them access to the more intimate details of family life in the teeming alleys and courtyards of this preindustrial slum. Chavatte himself was also forced to work for others and considered himself an *ouvrier sayetteur* even though, like his father, he had been received as a master. He was thus a rather ordinary representative of the *menu peuple* who lived through the plagues, wars, shortages, and unemployment of the age of the French conquest. What set Chavatte apart was that he was able to compose a chronicle of the life of his city in which he recorded firsthand, in his own colloquial language and phonetic spelling, all the ceremonies, incidents, mishaps, and gossip that interested him from 1657 to 1693. His unusual manuscript thus offers a rare glimpse of the mental universe of artisans in an important center of traditional cloth manufacture.

Alain Lottin's book is an analysis of Chavatte's chronicle and, by extension, a comprehensive study of the *mentalité* of people from his milieu. Lottin has

done an admirable job of remedying the anecdotal nature of this sort of document by checking out every reference, adding material from other memoirs, and extensively researching the matters described by Chavatte. The result is an original piece of scholarship, not just an editing job. Indeed, it is a rare pleasure to find a weaver's descriptions corroborated and filled out with official administrative correspondence instead of the other way around!

This study is therefore an invaluable tool for specialists interested in urban popular culture. I found it disappointing, however, because it confirms and illustrates many well-known generalizations about popular world-views without offering any real surprises. Chavatte's frame of reference was absolutely traditional. He thought in symbolic terms, attributing disasters to the will of God and believing in relics, magic, and witches. He was a devout Catholic who sympathized with the unfortunate. He was also a connoisseur of festive rituals, priestly oratory, and popular amusements.

Two interesting phenomena stand out. One is the way the Lillois retained their traditional loyalties—to the Spanish crown, the pope, the old-fashioned mendicant friars—long after the French annexation of 1668 and how they turned their preferences against Louis XIV, his Gallicanism, his wars, and his Jansenist bishop of Tournai. The other is the extent of Chavatte's economic particularism. He stubbornly defended the superiority of his guild's rather low-grade weaving "style" even against closely allied ones, and he opposed all economic concentration, believing that the size of workshops should be strictly limited in order to spread the work around. Chavatte's voice is worth listening to, and Lottin's presentation helps it to be heard. If the message is rather familiar, this may be only because of the time that has elapsed since 1968 when this study was defended as a thesis.

WILLIAM BEIK  
Northern Illinois University

CLAUDE-FRÉDÉRIC LÉVY. *Capitalistes et pouvoir au siècle des lumières*. Volume 2, *La révolution libérale, 1715–1717*. The Hague: Mouton; distributed by Walter de Gruyter, New York. 1979. Pp. 379. DM 72.

This second volume of Claude-Frédéric Lévy's study of French financial affairs, politics, and foreign policy in the early eighteenth century covers the first two years of the regency, a period of significant change in all these spheres. France's financial situation at Louis XIV's death, Lévy shows, was critical; there had been few attempts to reform either the fiscal or credit systems despite growing international awareness of the possibility of improvements in both. One of the reasons for this failure was the corruption of the political system; those



benefiting from the existing systems wielded great influence.

The "liberalism" of the regency consisted in its attempts to shake off these influences and introduce new policies in line with those practiced in Holland and England. Initially, traditional remedies were tried: devaluation of the *livre* in terms of metallic currency, a partial repudiation of the vast quantities of depreciated *billets de banque* circulating, and the establishment of a *Chambre de Justice* to examine the gestion of those involved in the state's financial affairs. These methods having failed, John Law's proposition for the founding of a private, national, note-issuing bank was accepted. Lévy describes in great detail the origins of this bank about which, until now, we have been ill informed. The foreign dimension of the regency's liberalism consisted in a rejection of pro-Catholic policies in favor of a rapprochement with Holland and England, a policy that was crowned with success when the Triple Alliance was signed in 1717. Lévy follows in meticulous detail the complex negotiations that led to this treaty.

The study ends with the foundation of the *Compagnie d'Occident* in 1717. This, too, was a liberal step for the company's charter, modeled on those of English and Dutch companies, represented a rejection of the monopolistic principle apparent in the charters of the French companies founded previously. Displaced during these years were the *frères Crozat*, the most powerful financiers in France in 1715. Lévy's book begins and ends with references to their situation. The unifying theme of the volume is the gradual displacement of the Crozats, and their like, from royal counsels.

Lévy is immensely knowledgeable about this period; he writes as though he had lived through it. He has carried out thorough research in both French and foreign archives and made particularly good use of notarial sources for piecing together the often clandestine activities of financiers. The book is written as an extremely dense narrative. Lévy doubles back when any important new figure is introduced and provides a short biographical sketch. His book, which makes difficult reading, will serve above all as a work of reference. Its utility as such is enhanced by the conscientious footnoting and full bibliography and index.

JAMES THOMSON  
University of Sussex

ROBERT DARNTON. *The Business of Enlightenment: A Publishing History of the Encyclopédie, 1775-1800*. Cambridge: Harvard University Press. 1979. Pp. xiv, 624. \$20.00.

This massive, ambitious work recounts the publishing history of the *Encyclopédie*, especially some of its

later editions, and attempts to set an example of how to write a history of book publishing. Whereas specialists have tended to concentrate on one facet of any book, Robert Darnton recommends that all aspects of a book be studied, including its origins, manufacture, marketing, diffusion, and influence.

Following these recommendations in his account of the *Encyclopédie*, Darnton combines the new research techniques of the French masters of *histoire du livre* with more traditional scholarship. An independent thinker, he freely criticizes practitioners of any technique. He relies not merely on the standard printed sources but largely on manuscripts. His greatest treasure-trove is the papers of the *Société typographique de Neuchâtel*, which include thousands of documents on the 1777-79 quarto editions of the *Encyclopédie* as well as documents on other editions. He quotes from these documents in detail and in the original French. Some readers might find this irksome; all will admire his skill in deciphering various handwritings and in piecing together a very complicated story while making it understandable and colorful.

Darnton stresses certain facets of the book trade more than others. Fascinated by business history, he devotes hundreds of pages to the publishers' contract disputes, espionage, and trade wars accompanying several editions of the *Encyclopédie*. The result is that his description of the publication of the quarto editions, of the 1778-82 octavo edition, and of the *Encyclopédie méthodique* will now be the standard accounts; they serve as his illustrations of the cutthroat competition of eighteenth-century publishing.

Darnton is also an excellent guide to how the quarto editions were produced. As a bibliophile and social historian he elaborates, for example, on the kinds of paper used, the methods of typography, the money printers earned, and their working conditions.

Moreover, Darnton provides valuable data on the sales figures and distribution of various editions of the *Encyclopédie*. He proves that it was a best seller. Although the first edition was very expensive and was bought mostly by the elite, the later, cheaper editions reached the bourgeoisie of much of France and sold widely elsewhere in Europe.

Other sections of the book discuss the contributors to the first edition of the *Encyclopédie* and the contributors to the *Encyclopédie méthodique*. Darnton compares the two groups with regard to such matters as their social and professional backgrounds and their reactions to the French Revolution. Until much more biographical research is completed on these Encyclopedists, generalizations based on prosopography can be considered only tentative.

*The Business of Enlightenment* is perhaps least informative about the history of ideas. Darnton says one cannot know what readers thought of the *En-*



*cyclopédie*. But we do know what some readers thought. For the first edition of the *Encyclopédie*, one can turn to John Lough's *Essays on the "Encyclopédie" of Diderot and d'Alembert* (1968). In over one hundred and fifty pages Lough provides comments about the *Encyclopédie* drawn from eighteenth-century books, pamphlets, and periodicals. There should be some remarks about later editions as well in similar eighteenth-century sources. Also, Darnton's book contains little about the contents of these encyclopedias. He says that five of the editions are versions of "Diderot's basic text" (p. 36). But what exactly is "basic"? The quarto editions, for example, are much less impious and learned than the first edition, partly because many of the most important articles and plates were excised by a hack editor. How much and in what ways was Diderot's great work vitiated in later editions? Darnton only touches on such issues, but they are important in estimating the impact of the *Encyclopédie* and the Enlightenment from 1775 to 1800, one of his central concerns.

For many years to come scholars will turn to Darnton's brilliant work. They will learn a great deal about the publishing history of the *Encyclopédie*, the eighteenth-century book trade, and French history in general. They will also find a model of how to study a book in its totality.

FRANK A. KAFKER  
University of Cincinnati

ALAN WILLIAMS. *The Police of Paris, 1718–1789*. Baton Rouge: Louisiana State University Press. 1979. Pp. xx, 328. \$24.95.

Police, in the Old Regime sense of the term, referred to an agency of administration as well as to the objects (and objectives) of administration. In the first part of his book, Alan Williams deals with the agency, component by component. Most of the material is familiar; not even the evocation of the assemblies of police is as ground breaking as Williams claims. But these chapters have the merit of constituting a more or less exhaustive *organigramme*, featuring not only the protagonists but also the less well-known actors as well (watch and guard, fire service, building inspectors, and so forth). In part two, the author treats the objects and objectives: various modes of social control and relief, sanitation, and others. Williams focuses upon those areas in which the police introduced improvements that made Paris "a better place than it had been." This perspective suggests yet another reason why the Revolution may have been superfluous. But it begs one of the most interesting questions that the author promised to address at the outset: the "impact" of the police on the regime itself.

A major weakness of this study is its abstractness and remoteness. The police remain faceless and elusive, from top to bottom. We learn virtually nothing about the men who shaped the institution decisively (Sartine especially), about the bureau heads (was Pisan a member of the farmer-general family?) and the treasurers (what about Rouillé's connections with the financial milieu?), or about the commissioners, who cry out for prosopographical investigation. Williams's distance from the *quartiers* of the capital leads him to exaggerate the infringement upon and diminution of the responsibilities and powers of the commissioners and to ignore their semi-autonomous corporate existence. He overlooks the crucial question of the relations between the police and the people of the *quartier* and their deeply ambivalent perception of each other. He underestimates the menace of worker insubordination and overestimates the significance of servant criminality. The author rightly construes the placement of guardposts near the markets as a guarantee of order. But because he extrapolates "reality" from forms and regulations, he fails to note that the guards were often a source of disorder in the markets. By a similar process of reification, he presumes—incorrectly—that workers and domestics did not carry weapons because the regulations invested the guard with a monopoly of violence. He cites fines as a form of dissuasion and punishment. Yet he never moves beyond the police sentences to ask whether the fines were ever collected, how they were collected, and what happened if they were not paid. The guild statutes might suggest that *sindics* and *jurés* needed "prompting" to exercise police, but in practice their overzealousness posed a greater problem than their reticence. The guilds were "locked against the intrusion of any newcomers," if one subscribes to liberal propaganda, but not necessarily if one counts corporate admissions. Can one infer policy priorities on the basis of the allocation of men and money? Because more men were needed to police the streets than to provision the markets, does it follow that authorities counted on "fear" more than on bread? Moreover, it is extremely unlikely that the data on police expenditures are complete.

It may be that the only way to put flesh on the police skeleton is to plunge into the "Y" series at the Archives Nationales. Williams dipped into it timidly, but that is understandable, for the Y is a labyrinthine swamp. One way to compensate would have been to draw upon the many *mémoires de maîtrise* based on the Y and other police documents directed by P. Goubert, E.-M. Benabou, and others. It is surprising that Williams did not exploit the large manuscript correspondence in the Bibliothèque Historique between Police Chief Marville and Police Minister Maurepas (part of which

S. Pillorget has just published). The author ought to have justified his heavy dependence on Lenoir's papers by subjecting Lenoir's testimony and ideology to critical scrutiny.

STEVEN LAURENCE KAPLAN  
Cornell University

ISSER WOLOCH. *The French Veteran from the Revolution to the Restoration*. Chapel Hill: University of North Carolina Press. 1979. Pp. xix, 392. \$27.00.

Isser Woloch's intention in his study of the French veteran's prospects and fate from the declining years of the Old Regime to the Restoration is to look at some of the larger questions in social history that might otherwise have been buried by excessive study of the local and particular, thus riveting attention upon the unchanging nature of social institutions and policies. What commands his attention instead are the possibilities for change that ensued from two intersecting phenomena. The first was the growth of a substantial force of the male population that was mobilized for war yet survived it. He estimates that there were some 150,000 pensioned veterans from 1792 to 1815. Second were the policies devised to deal with them under the dual forms of welfare and control. He concludes that the promise of change embedded in the egalitarian aspects of the law of June 6, 1793, which aimed at erasing distinctions of rank in the benefits accorded veterans, was short-lived and that the reaffirmation of paternalistic forms of authority was congruent with the political changes initiated by the Directory and carried further under the Consulate and Empire.

The very important information that Woloch assembles and the insights that he offers might have been tied more firmly to his larger ambition of understanding change and resistance to change had he looked beyond the Jacobin failure to some of the prevailing attitudes toward a significant part of the population. He rightly points out that the rationalization of the plight of the veteran was part of a movement to deal with many groups in the population that were increasing in size, and because of disabilities and distress of various kinds, consuming, rather than contributing to the development of, the nation's resources. All or nearly all the images of the poor, their threats to order, their presumed laziness and dissolute life styles, their resistance to disciplined forms of work, were, with one exception, duplicated in the official views of veteran psychology and social behavior.

What distinguished the veteran from the other dependent sections of society was, of course, the nation's debt to him, which demanded a rather different kind of treatment: recognition and decent material benefits. The first was continually being

undermined, however, by the deterioration of the second. The veteran generally did not fare well, or, if he did intermittently, he was continually made aware of his marginal position by the authorities who shunted him from the *Invalides* to other institutions, devised self-help schemes that depended on grants of land and capital, recommended forms of labor deemed appropriate for the physically disabled, or, under Napoleon, administered resettlement projects in "frontier" regions outside France where he was supposed to regain self-respect through tilling the soil and serving as an emergency combat soldier in case of military need.

By arguing that the state's assumption of responsibility for the *mutilés de guerre* reduced poverty levels, Woloch inadvertently brings us face to face with the almost certain knowledge that the volunteers and conscripts were probably already part of the marginal sections of the population and would have swollen the ranks of the poor and the sick had not the war intervened to litter the battlefields with their bodies and their severed and scattered limbs. Continuity is not proved by the revived paternalism under Napoleon. The change, there for us to see, is to be found in this earliest of the modern, large-scale schemes of care, geared to cost analysis, discipline, and the suppression of dissent. This may have been the war's supreme irony.

HARVEY MITCHELL  
University of British Columbia

IRENE COLLINS. *Napoleon and his Parliaments, 1800-1815*. New York: St. Martin's Press. 1979. Pp. 193. \$22.50.

Irene Collins's monograph deals with the bodies specifically granted the "legislative power" in the Napoleonic constitutions—how they were formed, how they functioned, and how Napoleon managed them. Thus she concentrates on the Legislative Body (*corps législatif*) and the shorter-lived Tribunate. The Senate and Council-of-State are treated as apart from "parliament"—which is technically correct.

Collins makes no attempt to alter the accepted view that Bonaparte dominated his "parliaments." However, she does offer a convincing rationale for his treatment of them. For example, it is usually assumed that Napoleon abolished the Tribunate in 1807 because its members were troublemakers—despite the fact that he had already unconstitutionally ousted twenty of them in 1802 and cut their numbers from one hundred to fifty in 1804. Not so, says Collins, who holds that he actually eliminated the Tribunate for the reason he stated at the time—it had lost its "utility." In fact, in 1807, the tribunes were straining to cooperate with the emperor, but

the system by which the Tribune debated legislation and then reported it to the Legislative Body (which voted without debating) had not worked. Napoleon thus integrated the remaining tribunes into the *Législatif*, creating a single-house legislature that both debated (by section) and voted.

The emperor still had to endure opposition in the *Législatif*. He preserved it, nevertheless, but not, says Collins, to maintain "a pretense of parliamentary government," as is generally assumed. He saw it as an essential "means of communication between the government and the public." He insisted that every new law be publicly introduced, explained, and justified by his councillors-of-state—and then justified again by legislators from the section that had debated it before the whole *Législatif* voted.

All this makes sense if one remembers that Napoleon was an authoritarian democrat who prided himself on governing *for* the people. The Legislative Body was a useful forum, therefore, for explaining why his laws were in the general interest (any opposition was expressed only by the raw number of negative votes, registered without comment). The "people" he wanted most to reach, of course, were the *notables* of the departmental electoral colleges and their peers.

Irene Collins's book is not without flaw, but she has made a useful contribution to the political history of the Napoleonic period.

OWEN CONNELLY  
University of South Carolina

MARIE FLEMING. *The Anarchist Way to Socialism: Elisée Reclus and Nineteenth-Century European Anarchism*. London: Croom Helm, or Rowman and Littlefield, Totowa, N.J. 1979. Pp. 299. \$26.50.

After many seasons of neglect, the career of Elisée Reclus has received an English-language biographical interpretation for the second time in two years. Marie Fleming's work is a commendable effort by a Canadian-based scholar to condense the varied ideas of the prolific geographer-anarchist into a single small volume.

Fleming's work follows soon after Gary S. Dunbar's *Elisée Reclus: Historian of Nature* (1978) and complements it in many ways. Dunbar offered an analysis of the scientific side of Reclus's career as a popular geographer; Fleming has focused her attention on the evolution of his anarchistic ideas. Both have made extensive use of family archives, socialist and anarchist files, and other primary material scattered across Western Europe.

There is little consideration of Reclus's achievements as a geographer in Fleming's work; this book alone would inspire little appreciation of the French scholar's influence outside the spectrum of anar-

chism. The last chapters are largely a montage of Reclus's ideas in the realm of politics; Fleming's difficulty in organizing her material is obvious. She has tried to fill the gaps in the historical evidence with informed speculation, which is identified as such. Like most biographers, Fleming obviously developed a deep affection for her subject; in the penultimate chapters she accepts the assessment of Reclus's contemporaries who regarded him as "the Anarchist Prophet" and "a saint." On the other hand, in the afterword, she concludes that his eschewal of organized political action was naive, and she accuses him of authoritarianism and insensitivity to human suffering.

Fleming argues that the decisive experience in forming Reclus's philosophy was the Paris Commune; his definitive anarchist position was developed after his arrest and imprisonment for participation in the events of March and April 1871. He became increasingly less tolerant of other socialist positions subsequent to those developments. Yet she argues, with dubious reasoning, that Reclus was more purely anarchistic and more supportive of freedom of thought than his famous anarchist colleague, Prince Peter Kropotkin.

One may quarrel with other generalizations that Fleming makes in passing, such as the assertion that Reclus "was perhaps the most progressive of all prominent nineteenth-century revolutionaries in his views on women nad marriage" (p. 244). But she has made a good argument for the idea that the anarchism of the last century was a remarkably stimulating intellectual leaven, whose force is not spent.

JAMES W. HULSE  
University of Nevada,  
Reno

ROBERT R. LOCKE. *Les fonderies et forges d'Alais à l'époque des premiers chemins de fer: La création d'une entreprise moderne*. Translated by ELISABETH-ANNE BENOIST-D'AZY. Paris: Editions Marcel Rivière. 1978. Pp. 298.

French industrial history had been enriched by a find of a large collection of the letters of people associated with the *Compagnie des Fonderies et Forges d'Alais* in southern France, a company based on nearby coal mines and iron ore. The letters cover the period of its founding in 1826 until it left the hands of the family in 1874. Robert R. Locke has produced a book based on this collection; it includes excerpts from 333 letters preceded by an 80-plus page explanatory text.

Although one could have wished that the introduction placed the company history a little more securely in the framework of the entire industry—there is, for example, no reference to Jean Vidal's *L'industrialisation de sidérurgie française, 1814-1864*

(1867)—the letters and text do illuminate many aspects of the industry as well as the company, covering technology, finance, recruiting of labor, finding outlets, and the like. There was a long period of teething troubles, and the firm never really made profits until the railway boom picked up in the 1840s and especially after the establishment of the Second Empire at the end of 1851. There is a revisionist view that French industry was thriving in the first half of the century after the Treaty of Vienna, but it is hard to find support for this view in the history of Alais. Technological help was sought by obtaining consultants and workers in England, as well as by trips there by Emile Martin, a consultant, and Benoist d'Azy, the central figure in the company along with the Paris banker Hippolyte Drouillard. Although the company had good connections to financial circles in Paris and Marseilles, its success waited on political help to Paulin Talabot's Paris-Lyons-Marseilles railroad and its antecedents; all hands in the Alais iron works turned out to influence the parliamentary vote on a 6 million francs loan for the railroad.

Of particular interest to this reader are the interruption of letters at the end of 1847 and early 1848 and the subsequent news that workers in Alais had rioted four times and had expelled the English workers, including one Shakespeare, who had been with the company since 1836. (A British railroad chief engineer with the Paris-Orléans railroad had been killed in one such riot.) Attacks on the persons of English technical staff help to explain why the British, so eager to invest in French railroads and in Alais in the 1840s, turned elsewhere after mid-century, compelling the French to create new financial means to develop the country.

A letter from Georges Dufaud of Fourchambault in the Nivernais lists the major iron producers in 1845: Alais is tied with Le Creusot at 12,000 tons of rails capacity per year, behind Decazeville at 15,000, and ahead of Hayange in Lorraine at 10,000. The total capacity in thirteen plants was 88,000.

Nuggets such as these make the book more than a company history, with many glimpses of the coal, coke, and iron industries and the industrialization process in France.

CHARLES P. KINDLEBERGER  
*Massachusetts Institute of Technology*

ALLAN MITCHELL. *The German Influence in France after 1870: The Formation of the French Republic*. Chapel Hill: University of North Carolina Press. 1979. Pp. xviii, 279. Cloth \$17.50, paper \$9.00.

Allan Mitchell's previous work has already established him as a leading specialist on Franco-Ger-

man relations in the Bismarckian era. His new book inaugurates his most ambitious project to date: it is the first volume in a planned trilogy that will seek to show that "the German question . . . vitally affected every major aspect of French public life after 1870." German influence in that era, he insists, was not just tangential or temporary but fundamental and enduring. The volume at hand treats the political and economic aspects of that influence, while its successors will focus on military, religious, and social developments in France. For intellectual influences, Mitchell refers us to Claude Digeon's massive monograph.

Robert Paxton has already demonstrated, in his fine book on Vichy, how much can be learned about France by digging through the German archives. The Vichy years, of course, were exceptional, since the Germans were in direct physical control. But Mitchell argues that both during and after the earlier round of German occupation in 1871-73, German pressures and threats weighed heavily on France's destinies. He contends that France became "a certain kind of republic" in considerable part "under the impact of German domination" and that the Third Republic's subsequent history continued to bear the marks of its origin.

Mitchell's case is vigorously argued and is buttressed by a great deal of solid evidence. He shows that Bismarck's pose as a disinterested observer of French domestic affairs was clearly hypocritical; Bismarck remained constantly and almost neurotically vigilant to detect the least sign of either *revanchisme* or political instability across the Rhine, and he resorted time and again to flagrant pressure and outright threats of renewed war in an effort to inspire "a salutary fear" among French leaders. Throughout the 1870s the German statesman intervened repeatedly to stabilize the moderate Thierist republic against its monarchist-clerical rivals; from 1877 onward he switched to support of a prospective Gambettist regime, first against the MacMahon forces and then against what seemed to be a new threat from the left. Although the precise impact of these various actions on French decision makers cannot be measured, Mitchell is convinced that it was considerable and that most historians have underestimated its importance in shaping both the political structure and the economic policies of the Third Republic.

Some readers may suspect that the author has pushed his case a bit too far. Still, the evidence that he presents from both the German and the French archives will not be easily refuted. He demonstrates beyond doubt that German interference and pressures were more often brutal than delicate and that French political leaders, "perpetually apprehensive" of what Berlin might do next, kept trying to reassure the Germans that they only wanted to be good



neighbors. By the end of the 1870s, Mitchell tells us, French leaders "were probably far freer of German control . . . than they realized," but their anxiety still persisted: "apprehension had become a well-conditioned reflex." The mood at Versailles in the 1870s, it would seem, had something in common with that at Vichy after 1940.

GORDON WRIGHT  
Stanford University

INGE SAATMANN. *Parlament, Rüstung und Armee in Frankreich, 1914–18*. Düsseldorf: Droste Verlag. 1978. Pp. 523. DM 86.

Inge Saatmann's study of the Senate and Chamber army commissions during World War I embodies the first sustained analysis of the French parliamentary committee system since Joseph Barthélemy and R. K. Gooch completed their work in the mid-1930s. Unlike her predecessors, Saatmann had available to her the commission records located at the Archives Nationales and the Palais du Luxembourg. She has examined these records with scrupulous care and integrated her findings with the vast literature on governmental structure under the Third Republic. The assembled material sheds light on the resilience of parliamentary institutions even under the most acute wartime strains, notwithstanding the mounting ineffectiveness of those institutions in coping with economic and social problems thereafter.

Saatmann endorses René Girault's controversial thesis that a dynamic entrepreneurial class existed in France before 1914. In large part she credits the business leadership for the nation's successful economic mobilization between early 1915 and the spring of 1917. But, she contends, oligopolistic industries in the metallurgical, chemical, and energy sectors misused their growing political influence. Their chosen instruments were the Senate and Chamber army commissions. These commissions, in Saatmann's view, came to dominate the political process, assuming the prewar roles of the parties and controlling the plenary secret committees. In the short run, the commissions prodded a laggard bureaucracy at the war ministry to fight a modern industrial war. But in longer perspective they constituted the vehicle through which the economic oligarchies took control of the state, undermined "democratic legitimacy," and set in motion forces leading to the disintegration of the Third Republic. The particular villain in this account is Clemenceau, who first in the Senate army commission and then as premier manipulated the patriotic campaign for resisting the Germans in order to undercut the Socialists at home.

The general argument appears more likely to impress kindred spirits in the Bremen school system,

where Saatmann teaches, than to persuade empiricist historians. The author seems unduly eager to find evidence of class struggle where contemporaries saw mostly desperation, confusion, and improvisation. No doubt German heavy industry did take advantage of wartime opportunity to impose a stranglehold on its society and economy, but it stretches credence to claim that a parallel development took place in predominantly agricultural France. Moreover, Saatmann's preoccupation with theoretical issues leads her to emphasize the constitutional features of debates about legislative oversight of the armies, when the overriding concern of those involved was how to pursue the war most effectively. Finally, her narrow research focus results in occasional errors of judgment or emphasis. She might, for example, have softened passages defending Malvy as interior minister if she had perused War Ministry records that demonstrate his complicity with defeatism.

The great contribution of this work lies in the specific information it provides. There are full accounts here of the workings of the legislature under wartime pressures; the legislature's relations with the executive, the press, and industry; and civil-military relations. Saatmann is notably lucid on the process of military procurement, and she illuminates the unfortunate Salonika expedition, the fall of Lyautey, the disastrous Nivelle offensive, and the fruitless negotiations for a compromise peace. The chapter on the secret committees of 1916–17 is particularly instructive. Students of Third Republic political and institutional history will want to read this book closely. The logorrheic style may strain the patience of nonspecialists, and the digressions on Balzac, Habermas, and Montesquieu will not appeal to those partial to a spare narrative line. The persevering will, however, feel amply rewarded by Saatmann's arduous research on a compelling subject.

STEPHEN A. SCHUKER  
Brandeis University

CLEMENS A. WURM. *Die französische Sicherheitspolitik in der Phase der Umorientierung, 1924–1926*. (Europäische Hochschulschriften, series 3, number 115.) Bern: Peter Lang. 1979. Pp. 746.

This monograph by a student of Gerhard A. Ritter, is a well-researched, carefully constructed, and (for the most part) closely argued account of the reorientation of French "security policy" in the Locarno period. Burdened by financial weakness and recurring currency crises, French governments jettisoned their "continental system" based on forceful execution of the Treaty of Versailles and East European alliances in favor of compromise with Germany and cooperation with Britain and America.



Clemens A. Wurm's major contribution is his analysis of the domestic and foreign economic context of French diplomacy. His thesis rests on extensive research in French archival sources, especially ministry files and private papers made available by the Quai d'Orsay since 1972.

Wurm's theoretical introduction defines *Sicherheitspolitik* as an integration of foreign policy, economic policy, military strategy, and such intangibles as misperception of a historical adversary, disjuncture between ends and means in strategic planning, and conflicts of priority between "national security" and the "domestic security" of ruling elites. Complete integration of all these factors would, in the author's view, permit an apotheosized *Totalgeschichte* (pp. 8–10). This is not yet possible, we are told, but the elements noted still serve to clue the reader into the many shortcomings of French security policies in the 1920s. This duty behind him, Wurm then eschews theory for an empirical, common-sense "sorting out" of the domestic and strategic factors shaping French policy.

The *Cartel des Gauches*, we learn, was never an "echte Realität" (p. 133). Rather, war and victory helped fracture the French left by "legitimizing" the republic, eroding the negative consensus around anticlericalism and antireaction, and sharpening social and economic issues on which the Radicals and Socialists parted ways. Thus the thesis that the cartel was laid low by the *mur d'argent* must be refined—divisions within the left and the financial incompetence of the cartel's leadership contributed. This collapse gave conservatives another opportunity to call for "national unity" and seek a foreign solution to avoid sacrifices at home, an "Instrumentalisierung aussenpolitischer Zusammenhänge zu innenpolitischen Zwecken" (p. 176). Although he shies away from a strict "primacy of domestic policy" interpretation, Wurm does assert the centrality of the continuing financial crisis in French policy from the Ruhr to the Dawes Plan to Locarno to Thoiry.

France's mid-decade dilemma arose from its inability to marshal the resources to support sanctions against Germany and play midwife to the new East Europe. By 1925, with the end of the respite after the Morgan loan and the London Conference, the cartel failed to fashion an acceptable fiscal program, and the franc sank anew. Meanwhile, the war debt obligations to the Anglo-Saxon powers still hung over France, industrial interests lobbied for renewal of the "natural" ties of French and Ruhr metallurgy, and domestic pressure rose for another lowering of the military term of service. This conjuncture of forces impelled a policy of detente. The payoff? Wurm argues that Briand formulated his "peace policy" leading to Locarno with the prospect of U.S. financial aid firmly in mind (p. 407). He then moves to a lucid account of the negotiations for

commercialization (public sale) of the German Dawes Plan obligations, which could have yielded considerable sums in reparations and eased the French currency crisis.

Poincaré returned to power in July 1926 and supported commercialization, hoping to open Western (especially U.S.) capital markets to the German bonds. Briand believed this would require German consent, hence revisionist concessions to Berlin. This set the stage for the climax of the French stabilization struggle and of Briand's Locarno policy—the meeting at Thoiry. Briand would seek Stresemann's aid in solving French internal problems, while concurrent negotiations between French and German heavy industry restored the prewar cartel ("das sichtbare Zeichen der Beendigung des 'Krieges nach dem Kriege'" and evidence of the "pazifistischen Schritt der Schwerindustrie" according to industry spokesmen [p. 443]). But Briand's initiative was undone by Poincaré's domestic stabilization package that removed the urgency for a grand reparations-revisions compromise with the Weimar Republic. The Thoiry proposals, which Wurm admits were half-baked (p. 552), met opposition from America, Britain, and the French army in any case, clarifying the international and domestic limits to Franco-German reconciliation in the 1920s. There would be no return to a power policy as advocated by diehards like Louis Marin, but neither would Briand's peace policy suffice to appease Germany. These interpretations are generally persuasive and very much in line with those of the unpublished dissertation of Edward Keeton, "Briand's Locarno Policy: French Economics, Politics, and Diplomacy, 1925–1929" (1975).

Finally, a quixotic complaint. Although the *Europäische Hochschulschriften* series performs a welcome service, it is nevertheless unfortunate that important works such as this come to us in the encyclopedic, undigested form of the dissertation. The 559 pages of typescript are excessive, the customary *Personenregister* is useless as an index, and the backnotes are a verbal maze. There is a point beyond which such aggravations cease to be "cosmetic" and begin to interfere with a publication's utility.

WALTER A. MCDUGALL  
University of California,  
Berkeley

BARTOLOMÉ BENASSAR. *The Spanish Character: Attitudes and Mentalities from the Sixteenth to the Nineteenth Century*. Translated by BENJAMIN KEEN. Berkeley and Los Angeles: University of California Press. 1979. Pp. xiii, 325. \$14.95.

This essay, originally published as *L'Homme Espagnol* (1976), is the work of a scholar famous for his many contributions to Spain's economic and social history

during the sixteenth century. In this volume, however, Bartolomé Bennassar sets for himself the difficult task of delineating the character of the Spanish people during the whole of the Old Regime.

He begins by presenting us with a series of "models," or portraits, of representative Spaniards that illustrate that this was a people whose ideal was the acquisition of fame and fortune with a minimum of effort. Predictably, the list includes a bullfighter and a conquistador while omitting women, merchants, and Catalans. This is followed by an attempt to define "the spatial-temporal frame in which Spaniards lived for three or more centuries" (p. 5) and a series of chapters that examine what Bennassar calls "the time for living." These include discussion of the Spaniards' special blend of intense religiosity and anticlericalism, their renowned distaste for labor and consequent pursuit of leisure, their pleasures with emphasis on their innumerable festivals, endless *tertulias*, and abundant sex, their "passion for honor," and, finally, their "complacent" attitude toward death. The conclusion is an effort to explain what (from a Gallic point of view?) is described as Spain's "cultural backwardness."

This kind of history, interesting and imaginative as it is, is fraught with danger, and Bennassar, who is aware of this, nevertheless manages to stumble into the same traps to which others who have written about national consciousness in historical perspective have fallen prey. Much of the book's evidence, for example, comes from the accounts of Belgian, English, and French travelers—the selection of which, looking at the notes, is highly arbitrary—whose judgments are frequently accepted uncritically. Similarly, the use of trial records of the Inquisition to describe Spaniards' religious attitudes and sexual proclivities makes for interesting reading, but it should also be remembered that the images offered by these documents are colored by the inquisitors' own prejudices as well as those of the persons who sought to make use of this institution. Bennassar also exaggerates the extent to which these records provide for a complete slice of Spanish life. He writes that these documents provide no evidence of feminine homosexuality (p. 207). And no wonder! Such cases were generally directed toward secular, not inquisitorial, justice and were then treated with a severity not at all in line with what he describes as Spaniards' "complacency" toward "sins of the flesh."

Difficulties in the use and interpretation of sources are compounded by those of focus. Essentially, this book is about Castile; Catalonia and the other peripheral "kingdoms" of Spain are neglected, possibly because many of their attitudes and mentalities were very different from those reflected in the author's Castilian "models." Considerable emphasis is also placed on what is described as

Spain's "unchanging milieu," but this only leads to interpretations that exaggerate continuity and underestimate change, especially with reference to the eighteenth century. Equally questionable is Bennassar's tendency to view Spaniards as a race apart, unique in their manners and mores. Much of what he writes about the Spaniards' love of ostentation, for example, applies equally well to Venetians; their "passion for honor" has its parallels among Englishmen in the sixteenth century and Languedocians in the eighteenth; and their attitudes toward death do not appear too far removed from those that Philippe Ariès has recently outlined for Europe.

Such criticisms, however, are mostly the product of my own attitudes (and prejudices) about Spaniards of this epoch. Bennassar has written a very personal book, and the vision he provides of Spain is that of a scholar with deep admiration and respect for, not to mention knowledge of, his subject. Benjamin Keen's translation, moreover, captures the intense, personal style in which the essay is written, although he probably goes a bit far when he repeats some of the author's original mistakes. Even so, this is a book that adds immeasurably to our knowledge about Spaniards' everyday life. At the same time, however, it reminds us that much more about this subject remains to be discovered.

RICHARD L. KAGAN

*Johns Hopkins University*

JUAN JOSÉ CASTILLO. *Proprietarios muy pobres: Sobre la subordinación política del pequeño campesino en España (La Confederación Nacional Católico-Agraria, 1917-1942)*. (Serie Estudios. Ministerio de Agricultura Secretaría General Técnica.) Madrid: Servicio de Publicaciones Agrarias. 1979. Pp. xxii, 552. 450 ptas.

This work focuses on one of the more problematical social classes in contemporary Spain, the small peasant proprietors of Castile and the North. Juan José Castillo is primarily a sociologist, but he has written a creditable history of the efforts of the Catholic Church in the early twentieth century to organize peasant proprietors into multiclass *sindicatos* designed to save them both from the impersonal forces of capitalism and from radicalization by the left. The instrumentality for this was the National Agrarian-Catholic Confederation (CNCA), and Castillo's book is basically a history of that organization from its founding in Madrid in April 1917 to its integration into Franco's corporatist state after the Civil War.

The author's conceptual framework is only moderately Marxist, but he incorporates into his work, with apparent approval, a great number of quotations taken from writers of rather more pronounced orthodoxy who subscribe to various timeworn ideas not excluding the notion of "fascism" as the "final

stage" of capitalism and also as the essential characteristic of the Franco regime. The central problem for the author would appear to be that Castilian and northern peasants did not behave as Marx (for example, in the *Eighteenth Brumaire of Louis Bonaparte*) suggested they should: they did not accept as their "natural ally and leader" the urban proletariat. To the author's continuing bemusement, the peasant proprietors of these regions perversely insisted, over several decades, on identifying not with the propertyless peasants below them but with the landed classes above. Christian ideas formed a powerful ideological cement between the agrarian poor and the rich, and the peasants let themselves be recruited into "vertical" unions that were effectively subservient to the interests of the large landowners. This recruitment, as the author says, was a triumph for Catholic social action. But the main feat of the CNCA was the pursuit, over many years, of policies that benefited the great proprietors while being carried out with the aid of the masses of small peasants. The ultimate "payoff" was that the small peasants of the Center and North went over almost en masse to the Nationalist cause when the Civil War came.

All of this—although faithfully chronicled by Castillo—appears to violate his belief in the "materialist conception of history," and he is never quite able to reconcile himself to the apparent non-primacy, in this case, of economic and class considerations, which may explain the diffuse and not especially illuminating character of his concluding section. Nevertheless, a great deal of work has gone into this substantial monograph. As the first extended essay on the subject of the CNCA, it is a useful and, within the limits suggested, scholarly contribution to our understanding of Catholic social action in Spain and its bearing on the origins and outcome of the Civil War.

GERALD H. MEAKER  
California State University,  
Northridge

FRANCIS BALACE. *La Belgique et la Guerre de Sécession, 1861-1865: Étude diplomatique*. In two volumes. (Bibliothèque de la Faculté de Philosophie et Lettres de l'Université de Liège, number 226.) Paris: Société d'Édition "Les Belles Lettres." 1979. Pp. lxxix, 638. 160 fr.

Study of the foreign policy of Belgium involves close examination of the diplomacy of the small state's kings as well as that of the foreign ministry. Although the two often supplemented each other, they sometimes diverged significantly. Francis Balace demonstrates how great this divergence was during the American Civil War, how dangerous it

was for Belgium and for the Northern states, and how skillfully Leopold I concealed his activities.

Motivated primarily by a desire to protect the tentative Mexican empire of his son-in-law, Maximilian, Leopold worked to persuade Britain and France to grant diplomatic recognition to the Confederacy. Permanent division of the United States would, he believed, weaken the support given to Maximilian's opponents from the North, whether in the name of the Monroe Doctrine or republicanism. When foiled by British reluctance, which had been stimulated by the Confederates' lack of success at Antietam, Leopold schemed to arrange European mediation that, he intended, would benefit the Confederate cause.

The official position of Belgian Foreign Minister Charles Rogier, who was not on the best terms with the king, was one of neutrality. This posture was consonant with Belgium's international status and Rogier's desire for Union assistance in Belgium's capitalization of the Scheldt tolls. It was adhered to despite the cotton and industrial crisis that, Balace suggests, may have been aggravated by overproduction in preceding years. Complications did result from purchases of arms by the contending factions and Union recruitment of mercenaries. Balace describes well how Leopold publicly appeared not to differ with his minister's policy and even received credit for calming the British during the *Trent* affair, when he was actually agitating for Britain to take belligerent action. Equally impressive are the sketches of the Union representative in Belgium (and head of the Union secret service), Henry S. Sanford, and of the even more vain Confederate representative, Ambrose D. Mann.

This thorough account will be definitive unless long-missing sources are discovered. The author's judgment is sound, the writing clear, the narrative well paced. Detailed documentation including extensive quotations is given in multiple footnotes. These provide a fine guide to the literature of the subject and insight into the thoughts and actions of the participants. Special mention must be made of the exhaustive bibliography and of the valuable essay on the location and history of the numerous primary sources consulted by the author on both sides of the Atlantic.

J. E. HELMREICH  
Allegheny College

H. VAN RIEL. *Geschiedenis der Provinciale Staten van Zuid-Holland, 1850-1914* [History of the Provincial Estates of South Holland, 1850-1914]. 's-Gravenhage: Provinciaal Bestuur van Zuid-Holland. 1979. Pp. 524.

This volume is divided into three parts, each of which is subdivided into chapters. Part 1 purports

to present the genesis and legal functions of the provincial government of South Holland. *Inter alia*, it tries to discuss the separation of South Holland from the Province of Holland in 1840, the social and economic conditions, and the electoral system of 1850. Part 2 is entitled "Balance" and part 3 "The Political Development of the Provincial Government, 1850-1914." The latter treats, or attempts to treat, such subjects as political events and a variety of other ill-defined problems. There is obviously a need to write a history of the various Netherlands provinces, but this volume will not serve as a prototype. At best it is a *mélange*, if not jumble, of data, anecdotes, quotations, and so forth. Much of the factual material is not without interest or significance, but no effort has been made to give it any degree of cohesion, organization, or structure. Instead, readers have to use their own ingenuity to extract from these pages some meaningful data.

Whatever this volume purports to be, it is most definitely not, as the title might suggest, a history of the provincial estates of South Holland. There is little information on the political events, debates, parties, or factions in the provincial assembly or the role the assembly played in the evolution of the province in the nineteenth century. Perhaps one of the more interesting chapters is the discussion on the creation of the province and the debates on the issue of dividing the once mighty province of Holland into two entities. The author's presentation, however, consists mostly of a string of quotations, and we learn little, if anything, about the reasons behind the decision to create the new province.

Of importance and interest are the data on social and economic conditions; from them we may learn something about sanitary and health conditions, taxes, wages, the system of poor relief, and so forth. But much of the data does not always directly relate to the province but to other parts of the country. Furthermore, we never receive a clear picture of the social and economic development of the province between 1850 and 1914. Nowhere do we find a good survey of the development of the important city of Rotterdam or of various leading industries.

In conclusion, the usefulness of this work is very limited. At best it could serve as some kind of reference work from which we can extract information for further study and writing of a real history of the important province of South Holland.

GERLOF D. HOMAN  
*Illinois State University*

PEKKA SUVANTO. *Die deutsche Politik Oxenstiernas und Wallensteins*. (Studia Historica, number 9.) Helsinki: Suomen Historiallinen Seura. 1979. Pp. 201.

Count Axel Oxenstierna, chancellor of Sweden for forty-two years, was one of his country's greatest

public servants. After the death of King Gustavus Adolphus in 1632 he guided Sweden through the minority of Queen Christina. Surprisingly, no full-length biography of Oxenstierna yet exists; the few studies of various facets of his career that are available (by Nils Ahnlund, Johannes Kretzschmar, Walter Struck, and Wilhelm Tham) are dated. The present volume on Oxenstierna's German policies in the years 1632-34, appearing only a few years after Sigmund Goetze's more general study of the chancellor's imperial politics, is a significant and welcome addition. Pekka Suvanto's aim is "to clarify what Oxenstierna regarded as the main goals of Wallenstein and how these influenced his political and military decisions" (p. 10). For his study this Finnish scholar has relied on Oxenstierna's voluminous published correspondence and on unpublished source materials from Stockholm, Dresden, and Merseburg archives.

The result is a very detailed and careful, albeit rather tedious, analysis of the chancellor's German policies in the period between Gustavus Adolphus's death at Lützen and Wallenstein's assassination at Eger. Suvanto's work will not alter our basic understanding of the period or its main antagonists, but it does fill in some of the gaps. The author reiterates the view, developed at greater length in his earlier *Wallenstein und seine Anhänger am Wiener Hof zur Zeit des zweiten Generalrats, 1631-1634* (1963) that the imperial general was neither an idealist nor a megalomaniac but simply a political pragmatist who wished to safeguard the security of his own house. He portrays Oxenstierna as a cool and rational political analyst, careful and controlled in his actions, yet able to act boldly and decisively when the appropriate opportunity presented itself. Although the chancellor sympathized with his coreligionists in Germany, he remained always first and foremost concerned with protecting Swedish security and interests in the Baltic Sea. He distrusted and became increasingly disenchanted with Wallenstein's peace negotiations in 1633. He was equally suspicious of the north German Protestant electors, in particular John George of Saxony and his general, Hans Georg von Arnim, who were seeking to eliminate Sweden's influence in Germany in order to allow Saxony to play a more important role. In dealing with these antagonists Oxenstierna revealed himself as a masterful diplomat who took charge of the very complicated and confused political situation that followed the untimely death of his king.

Suvanto's assessment of Oxenstierna's German policies is a bit too sanguine. Since the author is primarily concerned with relating the chancellor's actions to Wallenstein, he concludes his analysis somewhat prematurely with the assassination of the imperial general in February 1634, when Sweden's position in Germany seemed secure. In spite of his diplomatic talents, however, Oxenstierna was un-



able to prevent the very rapid deterioration of this position in the succeeding months. The battle of Nördlingen, early in September 1634, was a catastrophe that shattered the Swedish position in Germany, which the chancellor had nurtured so carefully during the preceeding two years.

BODO NISCHAN

East Carolina University

MICHAEL TOCH. *Die Nürnberger Mittelschichten im 15. Jahrhundert*. (Schriftenreihe des Stadtarchivs Nürnberg, Nürnberger Werkstücke zur Stadt- und Landesgeschichte, number 26.) Nuremberg: Stadtarchiv Nürnberg. 1978. Pp. liii, 229.

This dissertation, issued as a typescript in the city archive's monograph series, is an examination of the middle and upper social strata in Nuremberg during the fifteenth century. Completed at Erlangen University, presumably under the guidance of Hermann Kellenbenz, Rudolf Endres, and Wolfgang von Stromer, it is an intelligent attempt to provide a concrete structural analysis of the middling groups—essentially, the craftsmen and merchants—traditionally regarded as the core of German urban society. Since the secretive tax system in Nuremberg left no good series of registers with wealth assessments or even exact payments, Michael Toch has sought reasonably solid statistical evidence for economic and social stratification in two different sources, the salt provisions of the 1440s and an uneven group of extraordinary lists from 1497 to 1504 (the best is the Common Penny tax register). Acutely aware of the deficiencies of these sources, Toch is careful and modest in the generalizations he draws from them. The rather elementary quantitative framework is filled in with numerous non-statistical materials, and the entire study is informed by the perspectives of the newer social history as practiced both in Germany and abroad.

In contrast to the unfocused essay by Werner Schultheiss in *Städtische Mittelschichten*, edited by Erich Maschke and Jürgen Sydow (1972), Toch's dissertation is a systematic, problem-oriented analysis of Nuremberg society. Starting with definitions and generalizations that Maschke and others have proposed for towns with guild constitutions, Toch tests them in this major industrial and commercial center with a strong patriciate and no guild influence. The results bring no great surprises but offer a satisfying specificity on issues of wealth and occupational stratification, social status, the functions of minor officeholding, urban topography, rural land-holding, education, marriage patterns, charitable activities, and asocial behavior.

Among the propertied burghers of mid-fifteenth-century Nuremberg, Toch already sees a fairly clear differentiation between a lower middle class (com-

fortable but generally less wealthy handicraft people) and a rich upper middle group of merchants and more entrepreneurial craftsmen. This distinction was even sharper with the elimination of craft elements from the upper middle class by 1500, a development Toch asserts but does not demonstrate or explain clearly. He becomes increasingly concerned with the relationship between non-patrician merchants and those in the political elite; and, unfortunately, with this thorny problem the issue of distinctions between the middle and upper strata gets lost. This is the fascinating but elusive area where class and noneconomic status confuse one another, as Toch knows, but he has allowed it to blur his real focus on middling groups. As the author points to social instability and elite conflicts before the Reformation, the inadequacy of his evidence (like the unimaginative and sometimes unclear presentation of his tables throughout) is especially frustrating. But I do not wish to seem negative: historians far more experienced than Michael Toch or I have left these issues unresolved, and Toch has given us a piece of historical craftsmanship, examples of which have always made the city of Nuremberg justly proud.

GERALD L. SOLIDAY

University of Texas,  
Dallas

HELMUT NEUHAUS. *Reichstag und Supplikationsausschuss: Ein Beitrag zur Reichsverfassungsgeschichte der ersten Hälfte des 16. Jahrhunderts*. (Schriften zur Verfassungsgeschichte, number 24.) Berlin: Duncker and Humblot. 1977. Pp. 337. DM 88.

This study is not for the general reader. It grew out of a Ph.D. dissertation for the Philipps University at Marburg and retains something of the narrow focus so often characteristic of doctoral works. But for those scholars interested in institutional history, especially that of the German Imperial Diet, it offers interesting insights into the workings of the Reichstag and, by extension, into the corporatist movement of the early sixteenth century.

Helmut Neuhaus examines a relatively obscure subcommittee of the Reichstag, the *Supplikationsausschuss*, a kind of petitions board that flourished during the reign of Charles V, 1521–55. The board, both in its membership and in the business that it handled, had its groundings in the early sixteenth-century reform movement begun in the Holy Roman Empire by Berthold von Henneberg. Neuhaus suggests that the fortunes of the board reflected the fortunes of the reform movement and thus serve as an index to corporatist strengths and limitations. The suggestion has considerable merit. Unfortunately, so much of Neuhaus's energies go into a description of the board—into blocking out the time



and place of its meetings, determining its membership, and sorting out its various agenda—that the linkages with the reform currents and the parliamentary life of the imperial estates remain tentative.

Certainly the volume of petitions handled by the board indicates that the contemporary Germans, poor as well as rich, sovereign as well as subject, were turning to both the Reichstag and the emperor for judicial, financial, and administrative assistance. By documenting this activity, Neuhaus sharpens considerably our perception of the term “Kaiser und Reich.” We see it now in concrete example as the reformed definition of imperial sovereignty. But what about the constitutional implications of the board’s decisions on these various petitions? After all, the board was but a subcommittee of the Reichstag. Its membership varied in terms of individual estate, but the number remained generally constant at fourteen: six electors, two spiritual princes, two secular princes, one Reich prelate, one Reich count, and two imperial free cities.

Insofar as individual petitions involved constitutional issues, this small group could not legislate; it could only recommend. And there was always the Kaiser, as well as the Reichstag itself and the other imperial institutions like the supreme court, who had a stake in any such constitutional recommendations. Neuhaus develops only a random few of the cases that came before the board and reports that the board handled the petitions in such a way as not to antagonize these other components of imperial authority. But it is not clear to me just how the board could act on any petition of consequence without colliding with these other loci of power. Now that Neuhaus has rescued the board from historical oblivion, we need someone to follow through the various cases and to trace out the actual implementations of the board’s rulings in those instances.

Meanwhile, we have Neuhaus to thank for focusing attention on an institution whose activities and membership demonstrate that the German estates could cross curial boundaries within the Reichstag and work together in a heretofore unappreciated fashion on the direction of imperial administration and government.

JAMES ALLEN VANN  
University of Michigan

CHRISTOPHER R. FRIEDRICH. *Urban Society in an Age of War: Nördlingen, 1580–1720*. Princeton: Princeton University Press. 1979. Pp. xvii, 350. \$22.50.

Nördlingen was a middle-sized imperial city of well under two thousand households, centrally placed north of Augsburg on the main trade routes out of

the Upper Danube Valley. Its economy was already stagnating before 1620. The Thirty Years’ War brought heavy inflation, arbitrary taxation, called *Anlagen*, and a reduction by one-half of the total number of citizen households. Recovery was remarkably swift in the two decades after the Peace of Westphalia. From the 1670s, war costs against the French and Turks imposed renewed *Anlagen* paid to the Swabian circle and Habsburg emperors. These burdens were heavier and more damaging than had been in their time the impositions of the Thirty Years’ War. Citizen craftsmen working with wool, leather, and flax were hit by local and regional stagnation of markets and subjected to the new capitalist competition of the putting-out industry, organized by unscrupulous merchants, emerging from the wool-weavers’ trade to flout the rules alike of mercantilist craft and of city council. It led to riots when combined with poor harvest and high cereal prices in the 1690s.

The introduction and chapter 1 locate civic politics and constitution within the town’s region, meager *contado*, and wider economy. Chapter 2 deals with the massive disruption to demographic growth caused by the 1630s, while chapter 3 shows how textiles and long-distance trade were damaged by war, leading to heavier investment in local victualing industries. Chapter 4 is an excellent archival study of wealth mobility, making skillful use also of well-known price and wage data from Augsburg to arrive at indexes of real values. Full tabulation is provided through appendixes of marriages, baptisms, burials, immigration, occupation, and mean, median, and total wealth, set against the wealth mobility of various fractions of Nördlingen’s productive citizenry and backed by many tables, graphs, and figures in the text. Chapters 5 to 8 deal with the mechanism of social control available to the city council, its treasury, church schools, and hospital, whereby the divine retribution of Lutheran paternalism was seen to hold the town together. Vivid vignettes are provided in the correspondence of a mother of six to her husband while she was waiting to be tortured and burned as a witch in 1590, as well as in the various visitations to the town’s six elementary German schools. Chapter 9 analyzes the Wörner family, whose new business methods led to modernity via strong opposition from a craft-centered council and citizenry.

Christopher R. Friedrichs provides a convincing study of urban development in seventeenth-century western Germany by demonstrating from the facts that the share of wealth held by the top 10 percent of the citizenry, relative to the rest, always increased at times of war and that the period of sustained peace in the 1650s and 1660s was the only occasion when wealth mobility favored poorer citizens. His thoroughly researched book draws Nördlingen into

the seventeenth-century crisis debate, and it moves that "crisis" from the 1640s into the latter half of the century by linking it to the rise of capitalism.

GERHARD BENECKE  
University of Kent at Canterbury

KARSTEN RUPPERT. *Die kaiserliche Politik auf dem Westfälischen Friedenskongress (1643–1648)*. (Schriftenreihe der Vereinigung zur Erforschung der Neueren Geschichte e. V., number 10.) Münster: Aschendorff. 1979. Pp. 438. DM 98.

Since the publication in 1959 of Fritz Dickmann's *Der Westfälische Frieden*, there has been a burst of interest and activity in studying the first great diplomatic congress of modern times. In addition to the impressive, though as yet incomplete, documentary publications of the *Acta Pacis Westphalicae*, there have been a number of monographic studies. Karsten Ruppert's work fits neatly into this scholarship and significantly augments it. His work on the theme arose as part of the project to edit the imperial correspondence for the *Acta*. The book is a revised version of a Bonn dissertation completed under Konrad Repgen. The study traces in close detail the emperor's diplomacy in Germany and at the peace congress from 1643 to 1648. It has all the virtues of classic diplomatic history: it is thorough, painstaking, virtually letter by letter in its progress. It clarifies the formation of Austrian policy, which went into crucial memoranda. It analyzes the political content of particular documents and indicates the byplay between court advisors in Vienna and the imperial envoys at Münster, especially the somewhat independent stance of the chief imperial negotiator, Count Maximilian von Trauttmansdorff. Of necessity, this detailed presentation makes for heavy reading, and the book is of much more value to the specialist in the areas of diplomatic history and Austro-German history of the period than the general history reader.

Ruppert has based his work primarily on the riches of the Vienna Haus- Hof- und Staatsarchiv but, in addition, has used for the first time the private papers of Trauttmansdorff, to which Dickmann was denied access. A close comparison of Dickmann's work with Ruppert's would seem to confirm the latter's conclusion that aside from personal detail and some anecdotal material, the private papers do not add substantially to the diplomatic papers in the state archive. Ruppert has also used materials in Munich, the Hague, Paris, the Vatican, and a few other places.

Particularly interesting are the careful development by Ruppert of the Habsburg policy on the cession of Alsace to France, which breaks new ground, and his conclusion that the last winter of

the negotiations, in 1647–48, was a decisive moment in the emergence of the princes collectively, rather than the emperor, as the chief negotiator for the Reich.

This is an excellent monographic study, which will serve not only as a guide to the forthcoming imperial correspondence in the *Acta* but also will be very useful for students of the Peace of Westphalia.

ROGER WINES  
Fordham University

PETER BEYER. *Leipzig und die Anfänge des deutschen Eisenbahnbaus: Die Strecke nach Magdeburg als zweitälteste deutsche Fernverbindung und das Ringen der Kaufleute um ihr Entstehen, 1829–1840*. (Abhandlungen zur Handels- und Sozialgeschichte, number 17.) Weimar: Hermann Böhlaus Nachfolger. 1978. Pp. 203. 21.00 M.

It is easy for the reviewer to criticize Peter Beyer's book because so much of the work is dominated by the ideological themes of Marxism. The book was written within the framework of this vision and makes continuous note of it as it follows the pronouncements of the collected works of Marx and Engels. Another bow is given regularly to the work of Hans Mottek, whose *Wirtschaftsgeschichte Deutschlands: Ein Grundriss* (1976) appears to have had a seminal influence upon Beyer. In chapter 2 of Beyer's book, for example, Marx and Engels are cited in twenty-eight of seventy-one footnotes, Mottek in eight.

Hardly a paragraph of this fairly interesting book is free of these conceptual bindings, making it difficult for the reader to evaluate the documentary evidence. The introduction and first two chapters are essentially a review of ideology in the framework of railway history. Later chapters examine the development of interest in a Leipzig-Magdeburg route, the influence of Leipzig merchants, the relationship between rail and water transport, and some of the diplomatic problems that arose when independent states, railway companies, and individual entrepreneurs had to deal with one another.

There are a number of worthy points. Friedrich List's influence in Saxony is developed in some detail, especially his connection with the Leipzig bourgeoisie. Leipzig itself appears to have struggled to attain a significant place in early railway development, and the Saxon middle classes, heretofore obscure in the literature available in this country, are revealed.

Of some value for railway historians is the list of archives and documentary collections consulted. This is revealing, in part, of the paucity of material available to scholars who are forced to limit their research to the DDR. As useful as their archives ob-

viously are, there are omissions that could have been corrected partially if the Eisenbahn Archiv in Nuremberg and the Haus- Hof- und Staatsarchiv in Vienna could have been consulted. One or two works published in the West are listed in the bibliography, including ones by Carlo Cipolla and W. O. Henderson, but little is found of the studies done in the Federal Republic of Germany during the 1950s, 60s, and 70s. This is not a work that every German specialist will wish to read. For most it will remain on the reference shelf.

DONALD S. HOFFMAN  
Edinboro State College

RAINER S. ELKAR. *Junges Deutschland in polemischen Zeitalter: Das schleswig-holsteinische Bildungsbürgertum in der ersten Hälfte des 19. Jahrhunderts; Zur Bildungsrekrutierung und politischen Sozialisation.* Düsseldorf: Droste Verlag. 1979. Pp. x, 597. DM 68.

The political history of Germany in the early nineteenth century owes much of its originality to the disproportionate participation in political movements of the *Bildungsbürgertum*—the teachers and professors, lawyers and officials, and other graduates of the classical *Gelehrtenschulen* and universities of the German states. The political importance of these groups during the Vormärz has long been noted, but historians have paid little attention to the problem of how the *Bildungsbürgertum* became an active political group. Rainer S. Elkar's study contributes to an understanding of this development by exploring the process of political socialization among the educated classes in one region of Germany—Schleswig-Holstein—in the first half of the nineteenth century.

Elkar's contribution to educational history is impressive. His straightforward analysis of quantitative data linking educational trends with broad structural changes produces some suggestive insights. He finds, for example, a lack of correlation between statewide levels of enrollment in higher education and both price series and population growth patterns, suggesting that the relationship between education and economic development was not as direct as was once suspected. He also finds that in this era of stagnating or even declining enrollments in higher educational institutions, social recruitment of the student body narrowed, resulting in a more pronounced "self-recruitment" of the educated professions and a more limited opportunity for social mobility through education. Although these findings are relevant for any discussion of the interests and political leanings of the educated, their implications are left largely unexplored.

Elkar rests his argument more heavily on the use

of sources that depict the inner world of the schools. He analyzes not only the official records pertaining to curriculum, conduct of classes, and discipline but also the extant records on student culture—minutes of student associations, lists of topics chosen for addresses and essays, student albums and memoirs. In fact, the tension between the official world of the school and the world of student culture is in Elkar's view one of the most interesting and dynamic aspects of student life in the Vormärz. Student efforts to create an autonomous intellectual life as a counterpoint to the repressive and stagnant official education produced a certain, if contained, capacity for intellectual innovation and even rebelliousness. The paradox of contained opposition is, of course, viewed as an important political lesson for the students, formative of their later political stance as well.

Throughout the study Elkar points to the peculiarities of the setting for his study. Schleswig-Holstein was economically backward, politically dependent, intellectually spellbound by the Enlightenment, and thoroughly Lutheran. Peculiar, too, was the version of liberalism espoused by the middle classes—more moderate, *Stand*-oriented, fearful of revolution than elsewhere. Similarly, the democracy favored elsewhere found little support in the duchies. Elkar would like to argue that there are connections between the backwardness of the economy of Schleswig-Holstein, the intellectual character of its schools, and the nature of its political life. But the effort to make specific links between educational and political history fails in the end. By nature a comparative argument, it requires analyses of other regions so that what is peculiar to this region can be sorted out from what was generally true. It would be surprising to discover that school discipline, for example, was vastly different in other German states, despite admittedly varying degrees of receptivity to curricular innovation. Yet other political tendencies did emerge in middle-class milieus elsewhere. Comparison would certainly help to make the case more clearly.

Part of the problem lies as well in the failure to deal seriously with influences outside the school. Although Elkar discusses both familial socialization and structural change, he spends little time on the changing interests and strategies of middle-class families as they coped with a changing world. The structural suggestions of the early chapters are never integrated into the intellectual and political arguments made later on. But as it stands, Elkar's study provides an important contribution to the intellectual history of the German middle classes and some insights into the political formation of the *Bildungsbürgertum* of Vormärz Germany.

MARY JO MAYNES  
University of Minnesota,  
Twin Cities

TONI OFFERMANN. *Arbeiterbewegung und liberales Bürgertum in Deutschland, 1850-1863*. (Forschungsinstitut der Friedrich-Ebert-Stiftung, Politik- und Gesellschaftsgeschichte, number 5.) Bonn: Verlag Neue Gesellschaft. 1979. Pp. 623. DM 108.

After Prussian troops had dispersed the last German rebels in 1849, there were still workers' clubs about. Some were of democrats, some were printers' and tobacco workers' unions, some only social groups. Toni Offermann has the most precise information on how the dozens of German states used the laws on the books to destroy these tag ends of revolution.

Politics resumed slowly in the fifties, the bourgeoisie being first back in the field. Both its Progressive Party and National Association had plans for laboring men. They were to be educated through lectures and courses so that they might be brought to a higher cultural and, with luck, economic level. But they were not to engage in political discussion, which did not concern them, and, above all, they were not to have anything to do with socialism and communism. An anxiety about such doctrines was already present on the German scene. Offermann provides the fullest set of facts on these educational societies with their bourgeois tutors and artisan pupils, including tables on the occupations of the participants.

Those who joined were unlikely to have been independent craftsmen, attached as the latter were to a vision of the past. Nor were they unskilled day laborers, who had little to gain from increased literacy. Rather, the few thousands who filled the ranks were skilled workers, shaken by having crossed the threshold of the factory and thereby having lost their freedom.

The middle-class politicians expected to keep their working-class children in separate cradles. Blue collars were not to discuss economic problems, politics were taboo, and men from different states were not to meet in national convention. This well-documented but patronizing effort was of short success (1859-62). Workers in Hamburg and Leipzig soon tried to start something that would be more distinctly their own and not subject to mentorship. If their efforts were modest and halting, they did persevere. They wrote to the forceful, colorful, and quite erratic Ferdinand Lassalle, and he tried to form a party from these disparate clubs of the politically alert laboring men.

Offermann's finest work lies in the detail on these early proletarian groups. No one will have to go down this road again. But when he gets to 1862, he argues the indispensability of Lassalle and helps his case by not providing a sufficient national or international context for the increasing working-class agitation in the 1860s. The heroic nature of Lassalle is an old Social Democratic fable, echoed by his most recent biographer, Na'amann, who is multi-

tudinally cited here. But on Offermann's own evidence, these political neophyte artisans were making good progress. With Lassalle added to their cause for a few months, they did not make much more.

This is organizational history and of no insignificant moment in German history. For these are the years bourgeois and proletarian functioned together in politics. Once they separated, they were not to come together again, and the split was fateful.

RICHARD W. REICHARD  
Loyola University,  
Chicago

ADOLF NOLL. *Sozio-ökonomischer Strukturwandel des Handwerks in der zweiten Phase der Industrialisierung, unter besonderer Berücksichtigung der Regierungsbezirke Arnberg und Münster*. (Studien zum Wandel von Gesellschaft und Bildung im Neunzehnten Jahrhundert, number 10.) Göttingen: Vandenhoeck and Ruprecht. 1975. Pp. 386. DM 62.

In 1848, the Communist Manifesto asserted that "the lower strata of the middle class—the small tradespeople, shopkeepers, and retired tradesmen generally, the handicraftsmen and peasants—all these sink gradually into the proletariat, partly because their diminutive capital does not suffice for the scale on which Modern Industry is carried on, . . . partly because their specialized skill is rendered worthless by new methods of production." Known as the *Niedergangsthese*, this postulate has haunted generations of German historians, most of whom have managed to find "incontrovertible" evidence in support of the Marxian theories of capitalistic accumulation and concentration and the corollary decline of the artisans and their crafts.

Now Adolf Noll, a student of Wolfram Fischer, has re-examined the manifesto's proposition within the context of the "second phase of industrialization," that period of profound structural change in Germany's economy and society between 1870 and 1914. Since aggregate data on such economic variables as employment, income, and capital stock are lacking, researchers from Schmoller on have generally relied on demographic analyses to supplement impressionistic studies of the artisans' plight. Noll, however, has devised an alternate approach in which he investigates "the exogenous and endogenous causes of the economic changes that affected small business (as he defines *Handwerk*) and the social, political, and ideological effects of these changes on small business itself" (p. 35). His testing grounds are the districts of Arnberg and Münster, representative of Germany insofar as the former exemplifies a rapidly industrializing area, while the



latter stands for an agricultural, preindustrial region.

It is a worthwhile departure from the traditional method. Noll not only manages to generate nearly two hundred pages—more than half the book—of quantitative and qualitative evidence; he also arrives at some significant insights into the reaction of small business to industrialization. Thus Noll finds that the *Handwerker* adapted rationally and successfully to the changing economic structure; their response to more capital-intensive competition, for instance, was to increase their productivity and raise their capital stock. But ideologically, small business tended to hang back, continuing to see itself as the loyal intermediary between the crown, the capitalists, and the “*vaterlandslose Gesellen*.” The reason, Noll argues, must be sought in the irrational self-image of the *Handwerker* that rejected the dynamism of an industrial system as alien to their collectivist world.

But, above all, Noll provides a stimulating example of what the “new” economic history can do in a situation that is as hard to quantify as it is easy to forejudge. Prefigured in Rosenberg’s study of the “great depression,” the heuristic use of economic variables to explain noneconomic tendencies and structures in society has been widened here into a *Korrespondenzprinzip* designed to uncover the mutual influences and interactions at work within a social class. It is an approach that deserves to be emulated much more widely in dealing with the convolutions of history as much as the paucity of evidence that we so often have to face.

UDO HEYN  
California State University,  
Los Angeles

WOODRUFF D. SMITH. *The German Colonial Empire*. Chapel Hill: University of North Carolina Press. 1978. Pp. xiii, 274. \$16.00.

Recent research on late nineteenth-century European overseas expansion has begun to bear fruit in the form of general surveys and collections of essays also on German colonial history. Rudolf von Albetini’s pioneering comparative study of European colonial rule overseas from 1880 to 1940 (1976) was immediately followed by three other extensive publications: a symposium on *Germany in the Pacific and Far East, 1870–1914*, edited by Paul M. Kennedy and John A. Moses (1977); Lewis H. Gann and Peter Duignan’s Euro-African study on *The Rulers of German Africa, 1884–1914* (1977); and the East German symposium on Germany’s colonial expansion in Africa, edited by Helmuth Stoecker (1977). Unfortunately none of these volumes appears in Woodruff D. Smith’s bibliography. So this new general

study on German colonial history, while praiseworthy, exhibits some shortcomings.

It is hazardous, moreover, to write such a study without first evaluating the records of the Imperial Colonial Office. These records are presently located in the central archive of the German Democratic Republic at Potsdam and have been used by many Western historians. Unfortunately Smith was compelled to rely on an incomplete microfilm collection of those documents found at the Hoover Institute in Palo Alto. Otherwise, Smith’s interpretation rests upon several microfilms available in the Library of Congress and the National Archives, a few documents published by the German imperial government, a number of publications (too few unfortunately) from the contemporary discussion of colonialism in imperial Germany, and, most extensively of all, the numerous works on German colonial history that have appeared in the last fifteen years as a result of intensive research by other scholars in the colonial office records. This is not to say that Smith’s work should be dismissed as merely secondhand historiography. Once the body of research has reached a point where the nonspecialist is in danger of losing his general view of the subject matter, comprehensive studies with independent lines of interpretation become necessary. The new research on German colonial history offers a superb basis for such a general history despite the fact that many questions still remain to be answered. This basis would have been more secure, however, if Smith’s bibliography, which lags approximately four years behind the current level of discussion, did not contain many gaps.

Nevertheless, such shortcomings reduce only marginally the value of this important and necessary study. Although it omits some central aspects, this is the first survey of Germany’s colonial empire since that by M. E. Townsend, published in 1930. During the last decade and a half, research on German colonial history has had two main concerns: (1) the domestic, socioeconomic, and ideological origins and problems of German imperialism and (2) the economic and social history of the colonies themselves. The focal point of Smith’s study is clearly the first of these concerns. It is more a history of German colonial politics than of the German colonial empire overseas. Smith deals with this task admirably by providing us with useful guidelines for orientation as well as fruitful new challenges. His book does not, however, satisfy the need for a comprehensive study of imperial Germany’s colonial history based on all available archival records and encompassing all of the economic, social, political, and ideological aspects of the problem both in Germany and overseas.

KLAUS J. BADE  
University of Erlangen



E. ROSENBAUM and A. J. SHERMAN. *M. M. Warburg & Co., 1798-1938: Merchant Bankers of Hamburg*. Translated by A. J. SHERMAN. New York: Holmes and Meier. 1979. Pp. xiii, 190. \$24.50.

M. M. Warburg and Company opened its doors in Hamburg on the first day of 1798, and almost two hundred years later it still flourishes. If the proverbial caution of generations of family proprietors ensured that the bank would never become a leading financial institution in Germany, it also enabled their concern to take in stride the competition of larger banks and a succession of crises in the marketplace. For its first hundred years, the firm was a modest concern, with the partners living over the bank, and as late in 1898 it had only thirty-six employees. But M. M. Warburg and Company enjoyed a solid reputation and served as the Hamburg agent for houses as powerful as the Rothschilds. Like the Habsburgs, the Warburgs often married strategically, and their nuptial tentacles embraced the Henriques clan in Copenhagen, the Frankfurt Oppenheims, the Gunzburgs in St. Petersburg, and the Kuhns and Loeb in New York. Until 1900, the firm's principal business lay in currency transactions and bill brokerage, but thereafter it organized or participated in large international issues. Max Warburg, the guiding spirit in the firm from 1893 until 1938, and his partners began to assume director's seats in corporations to which the bank rendered financial services. M. M. Warburg and Company's sound investment policy enabled it to survive the crippling inflation and speculative adventures of the 1920s. By his own admission, Max Warburg failed to appreciate the gravity of Hitler's accession to power in 1933. Almost at once, Warburg representatives were forced off corporate boards and deserted by old clients. In 1938, after valiantly struggling to keep the bank afloat and helping less influential Jews to emigrate, Max Warburg went into exile in the United States, where he died in 1946. The bank aryanized as Brinckmann, Wirtz and Company, returned to Warburg control in 1956, and in 1970 reassumed the family name.

In 1922, to mark the forthcoming one hundred and twenty-fifth anniversary of the firm, Max Warburg commissioned his friend Eduard Rosenbaum to write a history of the bank. The financial crisis put an end to the project, which Rosenbaum resumed fifty years later with the support of Max Warburg's son, Eric. Rosenbaum was able to take the narrative only to 1889 and entrusted the subsequent history of the firm to A. J. Sherman. Rosenbaum's contribution is urbanely discursive, while Sherman, to whose portion falls the meat of the story, is tighter but thinly spread. The period from 1900 to 1933, when the bank was a financial institution of some prominence with important political

connections, receives but a scant sixty pages, much of it suggestive, all of it undeveloped. This abbreviated account cannot accomplish more than to whet the appetite for a more thorough treatment of the bank's long history. At the least, the publication of the complete version of Max Warburg's memoirs, from which his *Aus meinen Aufzeichnungen* (1952) was culled, would be very welcome. A professional study of M. M. Warburg and Company, with a full depiction of its business transactions, corporate connections, and political ties would be the appropriate monument to a dynasty remarkable both for durability as well as accomplishment.

LAMAR CECIL

University of North Carolina,  
Chapel Hill

ALFRED D. LOW. *Jews in the Eyes of the Germans: From the Enlightenment to Imperial Germany*. Philadelphia: Institute for the Study of Human Issues. 1979. Pp. x, 509. \$17.50.

This book has its beginnings in a manuscript that Alfred D. Low wrote in Vienna during the late thirties. Its purpose at that time was to take issue with the claim of Nazi writers that most great Germans had been enemies of the Jews. Before the work could be published, however, the Nazis annexed Austria, and the author fled with the manuscript to America. Twice revised and rewritten in English, it has now appeared after more than forty years.

In certain respects the book still reflects its origins. The residue of a defensive, sometimes polemical, tone remains in some of the chapters—as if it were still necessary to rescue Goethe, Schiller, or Nietzsche from their Nazi perversers. At some points the author seems as intent on making excuses for the antisemitic aberrations of leading figures as on explaining their views. But in general, and especially in the foreword and the concluding chapter, a more sober assessment prevails, the apparent result of a very different perspective gained by living after the Holocaust and in a pluralistic society.

Taking in both Germany and Austria and spanning a century and a half from the Enlightenment to the year 1890, Low deals with scores of prominent and not so prominent individuals representing the political, intellectual, and literary elites. Scarcely a figure of the first rank is missing as virtually everyone had something to say about Judaism and the Jews. Their assessments, of course, varied widely. Some liked Jews but not Judaism, some only particular Jews but not Jews in general, some ancient Jews but not modern ones, some only orthodox Jews or only those who resisted conversion. Others had the opposite preferences. It was thus im-

possible for German Jews to find favor in everyone's eyes no matter what they did. Unfortunately, Low does not tell us a great deal about how Jews responded to the judgments that were passed on them, although the other side of the equation—Germans in the eyes of the Jews—is obviously integral to the larger picture of the German-Jewish relationship.

In assembling affirmations of Jewishness, Low might have been more careful to distinguish between those figures who genuinely appreciated Jews as Jews and others whose regard was limited to Jews who transcended their origins. A large proportion of the personal relationships that he discusses involved Jews who were either converts to Christianity or at least wholly alienated from any form of positive Jewish expression.

Despite these shortcomings, Low's book is a valuable compendium of views on an issue that was by no means peripheral to German thought and creativity. Judgments passed on Jews and Judaism were symptomatic of more general thought patterns and political ideals. Drawing upon an impressive array of primary sources and using the large literature of studies on the relationship of individual figures to the Jews, Low documents a remarkable range of attitudes, feelings, and literary images. Rarely did the Germans he examines reach the genuine humanitarianism of a Lessing, but neither did any but a very few even approximate the consistent hatred of the Nazis. Low succeeds in showing that, for all of their prejudices with regard to Jews, Judaism, or both, men such as Frederick the Great, Metternich, and even Fichte, cannot be seen as foreshadowing the ideology of the Third Reich. The principal antecedents of unambiguous, unqualified antisemitism, one must conclude, lie elsewhere than in the thinking and actions of the German and Austrian elites.

MICHAEL A. MEYER  
Hebrew Union College,  
Cincinnati

HEINRICH POTTHOFF. *Gewerkschaften und Politik zwischen Revolution und Inflation*. (Beiträge zur Geschichte des Parlamentarismus und der Politischen Parteien, number 66.) Düsseldorf: Droste Verlag. 1979. Pp. 504. DM 84.

This important study argues that Germany's socialist unions failed to develop into forceful allies of the Weimar Republic because of political behavior patterns established between 1918 and 1923. Less encompassing than the title suggests, it focuses on the General Federation of German Trade Unions (ADGB) and seeks to explain why Germany's largest trade union federation missed the opportunity to become a decisive bulwark of democracy. War and

revolution, Heinrich Potthoff notes, opened new fields of political activity for organized labor. But Carl Legien and the Social Democratic leadership of the ADGB, fearful of losing social and economic advances made during the war and overburdened with organization duties, concentrated on preserving the status quo. This defensive posture prevented the ADGB from recognizing and exploiting the dimensions of political activity created by the Weimar Republic. As a result, union leaders silenced local demands for sweeping social and political change by centralizing authority in their own hands and drastically curtailing internal union democracy.

In foreign affairs, a field of responsibility for which the unions were ill equipped, the ADGB loyally implemented government policies without seriously considering the consequences. Domestic politics offered a more familiar battleground, but here, too, the ADGB did not capitalize on the opportunities within the Social Democratic Party, parliament, and state bureaucracy. Sharp political divisions within the unions, traditional aversions to politics, and mounting public opposition to trade union influence discouraged political initiatives. At times of crisis, such as the Kapp Putsch, the socialist unions did intervene for the republic. Yet no coherent political strategy developed out of these emergency actions. By the time a program had begun to take shape, mounting inflation undercut union confidence and led ADGB leaders to rely on the government and the SPD for political guidance. The pattern of inactivity and indecision visible by 1923 continued throughout the 1920s and seriously handicapped socialist labor's defense of democracy at the end of the Weimar Republic.

Potthoff's skillful analysis illuminates the organizational bottlenecks, missed opportunities, and short-sighted pragmatic orientation that produced a pattern of passivity. It is organized topically and based on a thorough knowledge of current trade union scholarship, new archival sources, and extensive use of ADGB publications. Important new information on the scope of ADGB participation in foreign affairs is introduced as well as carefully drawn profiles of the 1918–19 membership, the radical opposition within the unions, and trade union representation within the SPD's parliamentary delegations. Closer attention could have been given to the political role of nonsocialist unions since their influence often weighed far heavier than membership figures suggest. The author's emphasis on World War I as the formative experience for early Weimar political attitudes also underestimates the importance of labor's pre-1914 traditions. Nevertheless, within the limits set by the author, this thorough, well-written study provides valuable new insights into the political behavior of Germany's

socialist unions during the early years of the Weimar Republic.

RENNIE W. BRANTZ  
*Appalachian State University*

WILHELM DEIST *et al.* *Ursachen und Voraussetzungen der deutschen Kriegspolitik.* (Das Deutsche Reich und der Zweite Weltkrieg, number 1.) Stuttgart: Deutsche Verlags-Anstalt. 1979. Pp. 764. DM 82.

Popular and scholarly works on Nazi Germany and the Second World War continue to pour forth in dismaying profusion. The book under review, 764 pages in length, is the first of a new ten-volume compendium, *The German Reich and the Second World War*, which is being produced under the auspices of the Office for the Research of Military History in Freiburg-im-Breisgau under the direction of Manfred Messerschmidt. According to the producers of this series, their work will be far more than a military history and will include a full discussion of political and economic developments in Germany and German-occupied Europe, as well as detailed analyses of how the societies of all European countries were affected by the war. The producers promise further that, because their authors have drawn on an immense range of sources, many of them heretofore unused or neglected, their series will contain information and details that will be new to specialists as well as to the general reader. The titles of forthcoming volumes indicate that this series will provide a straightforward chronological account of the course of the war and concomitant problems. An exception to this chronological approach will be volume 5, which will focus on the Nazi war economy and theories of territorial expansion (*Grossraumvorstellungen*).

This first volume of the series deals with the origins of the war and the measures taken by the Nazi government to prepare for it. Part 1 by Wolfram Wette examines militarist and pacifist ideologies in the final years of the Weimar Republic, the propagandistic mobilization for war, and the Nazi reorganization of domestic institutions for war. Part 2 by Hans-Erich Volkmann analyzes Nazi economic preparations for war; part 3 by Wilhelm Deist covers the armament and build-up of the Wehrmacht; while part 4 by Manfred Messerschmidt discusses Nazi foreign policy in the context of preparation for war.

The scholarship of all four authors is impressive, their coverage detailed and comprehensive. They agree that the Nazi government was preparing systematically for war and do not presume to offer radically different or original interpretations of Nazi policy. Neither specialists nor general readers will find nuggets of new information in this book that

are likely to alter their own interpretations of the Nazi era, but they will find brief and authoritative discussions of such problems as the attitude of the German churches, political parties, and labor unions toward war; emigration and resistance; the effectiveness of the Four Year Plan; and the timing of the decision to go to war. To judge from this first volume, this series promises to be a solid comprehensive history, a readable analytical chronicle for insatiable students of the Third Reich, and an encyclopedia of information for others compelled to deal with the subject.

NORMAN RICH  
*Brown University*

FRIEDRICH JERCHOW. *Deutschland in der Weltwirtschaft, 1944-1947: Alliierte Deutschland- und Reparationspolitik und die Anfänge der westdeutschen Aussenwirtschaft.* Düsseldorf: Droste Verlag. 1978. Pp. 512. DM 68.

Several scholars have scrutinized Germany's post-World War II economic recovery as an issue in the Cold War. Friedrich Jerchow's richly documented study presents a refreshing and persuasive interpretation of the same events from a German perspective. Adding massive evidence from the records of German commercial organizations and administrative organs to the usual diplomatic sources, Jerchow analyzes in depth the interplay of forces leading to the revival of West German foreign trade. Since that revival depended on resolution of the reparations question, which in turn depended on the ability of the occupying powers to treat Germany as an economic unit, the analysis touches every major issue of the period.

Because the war left the occupying powers in control of Germany, Jerchow's analysis begins with a definition of their war aims. Using familiar sources, he reaches familiar conclusions: the Soviet Union sought security and economic recovery, Britain a stable European order, and the United States a liberal democratic world. The only novelty is the strength of the determination to regain great power status he attributes to France.

The originality and significance of the work stem from the conflicts among the aims of the occupiers and with actual conditions in Germany explored in the book's second part, aptly labeled "the confrontation with reality." Jerchow synthesizes previous studies of the occupation, the memoirs and papers of occupation officials, and his own extensive research in the records of German trade associations. The results are extremely instructive. The German material reminds us that the occupied had definite ideas about their fate and that these ideas had some influence on the occupiers. In fact, Jerchow contends that the leadership assumed by vari-

ous municipal and economic associations paved the way for the eventual development of a pluralistic society in West Germany. Second, Jerchow's detailed description of the administration of foreign trade in the British, American, and French zones reveals vast differences in both aims and methods. The British dilemma resulting from the need to revive an economy whose exports competed directly with British goods led to early efforts at cooperation between occupier and occupied. The American military government, fearful of lingering Nazi influence, postponed such steps and imposed extensive administrative machinery instead. In spite of the differences, the heavy burden borne by both Britain and the United States supplying food to their zones led to fusion as part of the effort to promote exports to balance the necessary imports. The French zone, on the other hand, proved so profitable to the occupier as to make unification of the western zones economically undesirable even when Soviet conduct made it a virtual political necessity.

Given the interplay of these forces, the eventual emergence of a separate West German state dependent on world trade should occasion little surprise. Jerchow recognizes the arrogance and omnipotence of American influence as well as the intolerable Soviet behavior that left Western Europe no alternative to American dominance. The subtlety of his interpretation, however, which is far more comprehensive than previous studies of these events, shows that all problems are not invented, and certainly not resolved, in either Washington or Moscow.

ROBERT J. GIBBONS  
*American Institute for  
 Property & Liability Underwriters  
 Malvern, Pennsylvania*

ROLF STEININGER. *Deutschland und die Sozialistische Internationale nach dem Zweiten Weltkrieg: Die deutsche Frage, die Internationale und das Problem der Wiederaufnahme der SPD auf den internationalen sozialistischen Konferenzen bis 1951, unter besonderer Berücksichtigung der Labour Party.* (Archiv für Sozialgeschichte, Beihefte, number 7.) Bonn: Verlag Neue Gesellschaft. 1979. Pp. ix, 433. DM 78.

Rolf Steininger begins his excellent monograph on Germany and the Socialist International after the Second World War by examining the roots of that problem in the interwar years. At the Paris Congress of the Labor and Socialist International (LSI) in August 1933, the German socialists were accused of suffering from a paralysis of will at those decisive moments when revolutionary methods were called for. Steininger correctly concludes that the LSI should have pronounced that verdict on itself. But internal splits and a growing retreat from inter-

nationalism made even a serious appraisal of LSI powerlessness impossible.

Following the first spontaneous (not Moscow-inspired) popular front negotiations between French socialists and Communists in 1934, a strong right-wing conspiracy developed in the LSI under the leadership of the British Labour Party. Its aim was to reconstruct the International in order to turn it into a powerless information bureau. Despite heroic warnings by the LSI Secretary, Friedrich Adler, that a second August 1914 capitulation was imminent, the triumvirate of British, Dutch, and Danish socialists had their way. The international working class, bureaucratically excluded from decisions taken at the top, was once again set adrift, disunited and helpless in a world at war.

The fate of socialist refugees in Britain after 1939 was pitiful. Those who were not won over to Labour Party self-aggrandizement, and still strove to give working-class internationalism some meaning, were treated as lunatics and outcasts. William Gillies, secretary of the Labour Party's international department, worked deftly to increase anti-German sentiments in his own party. In the climate of rabid German hatred he helped to create, the Nazi hierarchy and the German working class came to be viewed as the same evil.

Germanophobia loomed large when discussions about the reconstruction of the International began as the war was drawing to a close. Steininger traces the tremendous potency of that particular brand of poison during the postwar years, when the International was being reconstructed. It took a half-dozen years to arrive at an agreement on two difficult problems: how to integrate the German socialist party, whose reputation was tarnished by its past, and how to give a new face to international socialism. The German question was solved when Ollenhauer replaced Schumacher as party leader and when the party earned its anti-Communist spurs in the first years of the Cold War. At the founding congress of the Socialist International in Frankfurt in 1951, the Labour Party's historical preference for pragmatism over theory and suspicion of substantive internationalism played a decisive role. Steininger ends his introduction with the thought: "The history of the LSI is a history of its decline, which began soon after it entered the world-political arena" (p. 10). One might well ask whether the same judgment might not be made about international socialism's third incarnation.

This is a carefully researched and judiciously reasoned monograph, qualities one has come to expect of Friedrich-Ebert-Stiftung publications. Some two hundred pages of pertinent documents enhance the usefulness of the volume.

HELMUT GRUBER  
*Polytechnic Institute of New York*



KLAUS GÜNTHER. *Sozialdemokratie und Demokratie, 1946–1966: Die SPD und das Problem der Verschränkung innerparteilicher und bundesrepublikanischer Demokratie*. (Forschungsinstitut der Friedrich-Ebert-Stiftung, Politik- und Gesellschaftsgeschichte, number 6.) Bonn: Verlag Neue Gesellschaft. 1979. Pp. 300. DM 46.

German Social Democracy throughout its venerable history has been the focus of innumerable studies ranging from autobiographies by its leaders to analyses of its programs and goals. Undoubtedly, books on this important party, presently the senior governing one in the federal republic, will continue to attract the attention of scholars in the future. Such interest is enhanced as a result of the sponsorship of a number of studies by the Research Institute of the Friedrich Ebert Foundation (in Bonn), closely linked to the Social Democratic Party.

The present volume, falling under these auspices, deals with one crucial time segment, 1946–66, when the organization emerged from the ashes of the Nazi regime and became the chief opposition in the Reichstag to the Christian Democrats. Within this time span, the emphasis is on an analysis of the degree of intraparty democracy within the broader framework of a democratic political system.

In the introductory theoretical section, Klaus Günther distinguishes between the policy-making role played by party leaders (called the representatives) and the passive role of members (the represented) in the numerous groups of the highly structured organization. He seeks to determine the extent of intraparty democracy by examining chiefly the nature and fate of resolutions introduced by lower functionaries at the periodically held conventions. But he also seeks to assess the role of the SPD as an opposition party in the Bundestag to measure further the democratic nature of the German body politic.

In the substantive sections of the book, the author (presently teaching at the University of Bonn) is critical of the leaders' failure to promote discussion and allow for criticism of policies. "Stimulative" initiatives are tolerated, but serious dissent is stifled as leaders constantly seek to maintain unity in order to gain more voter support in state and national elections. Günther pleads for a broadening of a democratic "corridor" in which the rank and file has an opportunity to participate to a greater extent in the formulation of policy and in which the leaders consult the members more frequently. Surprisingly, in the light of his extensive treatment of intraparty democracy (or the lack of it), Robert Michels's iron law of oligarchy is alluded to only once.

As to the relationship of SPD to the German democratic polity, Günther focuses on various phases of the party's role in opposition: from con-

flictive (1946 to 1955), to cooperative (1955 to 1959–60), to adaptive (1959–60 to 1966). From this perspective, he examines the party's position on selected foreign, economic, and social policies and its "fixation" on elections, the role of parliament, and the rule of law as instruments to promote democracy. He concludes that any dissenting voices within the SPD who challenged their leaders' position on these issues had little impact, proving once again that the democratic ethos needed reinforcement.

Although the study is based on a wealth of documents and other archival material, the reviewer wishes that they had been supplanted by interviews with elite and rank-and-file members to provide for a more varied texture and a less jargon-filled text. The book enriches the literature on the SPD but leaves unanswered the question whether the author would stick to his basic thesis of a lack of intraparty democracy if he were to analyze the party from 1966 to the present when major factional and ideological schisms between the young Socialists and the old guard erupted periodically to make life for the leaders more difficult than in the first two post-war decades.

GERARD BRAUNTHAL  
*University of Massachusetts,  
Amherst*

HANS GEORG LEHMANN. *Der Oder-Neisse-Konflikt*. Munich: Verlag C. H. Beck. 1979. Pp. 294. Paper DM 39.

Hans Georg Lehmann's case study of the Oder-Neisse conflict is actually three studies of this important postwar problem: (1) a political/international-relations-oriented narrative of the sequence of events concerning the western Polish border question from 1939 to 1972 (with emphasis on the decisions made between 1944 and 1950); (2) a brief (pp. 177–83) summary periodization of the turning points in the sequence; and (3) an attempt to reformulate the known facts into a mathematical model of a conflict situation, using the tools of computer language (Fortran) and math-oriented political science model systems including such constructs as the interdependence of variables, feedback, the Boole Function mechanism, and the like. He has more success in realizing his goal, "die Verflechtungen internationaler, aussen- und innenpolitischer Interdependenzen zu erhellen," in the first two instances than in the last.

This is the first case study devoted solely to the Oder-Neisse conflict and as such is of value not only as a consideration of the theme but also as an analysis of how the Cold War affected international questions that had at one point or another been



considered essentially settled (at Yalta and Potsdam) but which were reopened and inflamed as the former Allies in the anti-Hitler coalition broke into competing power blocks, dragging their former German enemy with them.

Lehmann has been unavoidably influenced by the work of the revisionist historians of the Cold War but has stayed away from the various forms of economic determination that informs much of their work. Indeed, most of his book is in essence a well-done synthesis of straight political history. Nonetheless, one of Lehmann's most intriguing points will not please most traditional historians of the period, namely, that much of Soviet behavior and decision making was a reaction to Western, especially American, policy making (such as Byrnes and Marshall's revision of the U.S. position on the Oder-Neisse question taken previously at Yalta and Potsdam) and not an expression of aggressive expansionist tendencies (pp. 136, 142, 203).

One of the more interesting aspects of Lehmann's study is his examination of the effects of policy made by the super powers on Poland: the clear evaluation of Poland's domestic and foreign policy actions to strengthen its western border both legally and in terms of public opinion. He shows how the conflict and its ramifications drove the Poles further into the embrace of the Soviet Union often against their own wishes (see especially p. 158). Lehmann also examines in some detail what the various stages of the conflict meant to the different participants and how those governments manipulated and interpreted the circumstances to channel domestic political conflicts into foreign policy questions with regard to the border problem (pp. 128, 175).

In short, the first part of the book (pp. 15–176) is a well-documented (though the available state department and U.S. military government records were not consulted) evaluation of the subject matter, which is of value to the general reader and the specialist. The value of the third part of the book is more problematical. It is essentially a recapitulation of the first, attempting to explain the same events through the use of graphs, flow charts, tables, and computer language in a phased conflict model. While there is supplementary information and interpretation here not available in part 1, it is often hidden behind the veil of Fortran and the obscurantist text of a historian trying to build a mathematical model. This attempt obscures more than it clarifies, at least for this reviewer. Had Lehmann combined the first and third parts of the book without the model building he would have given us an even more comprehensive work of greater value.

There are also a few relatively minor negative points: a working knowledge of Latin phraseology will be helpful in reading this work; American military government was not trapped by its major directive (JCS 1067) but rather ignored it whenever

necessary (pp. 65–66); and it is questionable whether John F. Kennedy ended the Cold War in 1961 (p. 181).

The book is of considerable value for its clear analysis of the conflict in the first part and may be of use to cliometric historians in its third part.

BREWSTER S. CHAMBERLIN  
*Institut für Zeitgeschichte,*  
*Munich*

ALFRED PERRENOUD. *La population de Genève du seizième au début du dix-neuvième siècle: Étude démographique*. Volume 1, *Structures et mouvements*. (Mémoires et Documents, number 47.) Geneva: Editions Société d'histoire et d'archéologie de Genève; distributed by Librairie A. Jullien, Geneva, and Librairie H. Champion, Paris. 1979. Pp. xv, 611. 48 FR.

Many historians will not share Alfred Perrenoud's view that demographic data provide the key to social history. But students of his work will readily acknowledge the care with which he has selected, explicitly assessed, and analyzed his sources for a general description of the sociodemographic base of Genevan history covering the years 1550 to about 1830. Building on Edouard Mallet's *Recherches* (1837), Perrenoud reconstituted a 10 percent sample of 5,000 families between 1625 and 1780. Notarial records provided particularly valuable information on wealth, occupation, and the geographic and social origin of spouses for a representative cross-section.

In this first of the projected two volumes, Perrenoud intends to reveal population dynamics by isolating factors of growth and decline and by describing the role and structure of the migratory sectors, the composition of households, and socioprofessional patterns; thus "permanencies" are to become visible, "less for retaining what remained than for grasping what evolved" (pp. xi–xii). This is achieved, first, by a numerical overview, then by an investigation of structures, that is, divisions as to sex, marital status, and age, household composition, and occupational stratification. The third, perhaps most unique section focuses on "immigration . . . foundation and fabric of all urban demography" (p. 231), with a special description of the highly volatile alien work force. The last part concentrates on movement: of prices and income; of marriage, birth, and death rates reflecting seasonal, pestilential, and infant mortality. Graphs, lists, tables, and maps throughout the text and also in an extended appendix provide information at a glance.

Perrenoud concludes that for cities "all or almost all" demographic patterns still await discovery (p. 497) but are clearly linked to social structures. The term social is throughout perhaps too narrowly understood as merely involving income, wealth, and

occupation. Perrenoud's further conclusion that "the shock of events" (p. 498) does not truly alter basic patterns needs re-examination in the light of E. William Monter's discovery of a lasting shift from a "transitional" to a "completed" marriage pattern that was initiated by the famine and war of 1586-87 in the countryside and, after 1590, in Geneva proper (*Journal of Interdisciplinary History*, 9 [1979]: 423-25).

As a longitudinal study of evolutionary demographic dynamics in a middle-sized, north European city, undertaken in the tradition of French historical demography, Perrenoud's work is unique, thorough, and outstanding. As history it remains open to challenge. May the human past really be reduced to quantitative aggregates, given the irreducible core of people acting, at least to some degree, in freedom? Only patient inquiry into individual lives—with Pascal's "esprit de finesse," not just "de géométrie," as Jacques Barzun has stressed (*AHR*, 77 [1972]: 54)—will grant glimpses of that reality, perennially central to Clio's domain but truly beyond the grasp of merely numerical methodologies.

LEO SCHELBERT  
*University of Illinois,  
Chicago Circle*

R. J. W. EVANS. *The Making of the Habsburg Monarchy, 1550-1700: An Interpretation*. New York: Clarendon Press of Oxford University Press. 1979. Pp. xxiii, 531. \$49.50.

Following his excellent study of the bizarre world of Rudolf II (1973), R. J. W. Evans has undertaken the enormous task of explaining the emergence of the Austrian Empire and the forces that gave it some measure of coherence in spite of all the contradictions it embraced. He brings to the work an extraordinary command of the sources, Czech, Magyar, Rumanian, and Polish as well as those in the Western languages.

The outline of his thesis, based largely on intellectual currents, can be broadened to cover the social and political developments in Central Europe as well. There was, he argues, a three-stage development. First, the sixteenth century witnessed the gradual emergence of humanist culture in an open atmosphere of social opportunity, a movement that was suddenly interrupted by the Reformation and the onslaught of Habsburg political consolidation. By 1600 the feudal structure of society and the new political system had collided, overlapped, and to some extent coalesced. There then followed a period of crisis, roughly corresponding to the Thirty Years' War, that saw the triumph of the Counter Reformation and at the same time the emergence of a new class of magnate nobles in the three main re-

gions of the Empire. Confirmed in their position rather than created by the dynasty, this largely native magnate class held a virtual monopoly over provincial and regional affairs. This predominance was made all the more effective by their control of ecclesiastical and monastic offices through ties of family and patronage. Finally, there arose in the late seventeenth century an intellectual synthesis, the Catholic culture of the imperial baroque, that offered a set of attitudes and provided the necessary universal abstractions that gave sense and purpose to the whole enterprise.

Evans regards the political, military, and diplomatic events of the period as "epiphenomena" adequately treated in existing literature, and says very little about them. He concentrates instead on formal culture to show how society sustained the dynasty's universalist ambitions. In his analysis, the role of the Church, more Habsburg than papal, is not merely crucial but determinant: "it was Catholic orthodoxy which created Austria" (p. 191). After describing in great detail the magnates' patronage of the Counter Reformation, Evans attempts to anatomize the cultural synthesis this world produced. In the final section, he draws on themes developed in his work on Rudolf II: occult humanism, "official magic," and the persecution of unofficial sorcery. This offers many provocative insights into the thinking of a world seemingly impervious to the revolutionary ideas beginning to take hold in the West, or at least a world very tardy in taking note of them. His contrast between a "rational" West and an "irrational" culture in Central Europe seems to me rather overdrawn. The "enlightened" West had then, and still has, its full share of successful charlatans, wizards, and the like. Habsburg patronage of such types was hardly original or unique, though Evans does make a forceful case for a special interest in them in Leopoldine Austria. In any event, he has unearthed an imposing array of alchemists, astrologers, and eclectic pseudo-orientalists.

Though it is very well written, this is not a book for beginners. The arguments are tightly drawn, the detailed evidence is sometimes intimidating. It is a major contribution that will lift our discussion of the rise of the Habsburg monarchy to a new level of sophistication.

JOHN P. SPIELMAN  
*Haverford College*

JOHANN FRANZL. *Ferdinand II.: Kaiser im Zwiespalt der Zeit*. Graz: Verlag Styria. 1978. Pp. 384. DM 36.

Modern biographers, by and large, have not found the Habsburgs to be especially rewarding subjects. Powerfully but conventionally pious, not overly given to imaginative reflection, and only intermittently self-revealing, the dynasty was rarely as

exciting as the events that it helped shape and direct. The result is that any historian who tries to deal with the Habsburgs as human beings ends up by either concentrating on the historical context that surrounds them or, through authorial commentary, investing them with a kind of dramatic quality that most of them lacked.

Johann Franzl's work, which blends serious history with a touch of *Feuilletonistik* in the way Golo Mann did with Wallenstein several years ago, is decidedly of the latter order. His study of Ferdinand II, the devout but occasionally pragmatic mediocrity who presided over the outbreak of the Thirty Years' War and much of what followed, is an adequate recitation of the major events of the Emperor's life. The re-Catholicizing of his native Styria, the Habsburg *Bruderzwist* and his rather ambiguous part in it, the war and its Wallenstein interludes are all there. Franzl attempts to enliven this narrative with his own moralizing reflections on the brutalities associated with religious bigotry and the wars and torments that attended it, all of which were certainly functional aspects of Ferdinand's policies. Curiously enough, the author ends by urging historians not to proceed judgmentally, advice that he is hard put to follow in his own text. The result of his efforts is a book that is interesting enough to read, repetitious and cliché-ridden though it is. It also tells us little about Ferdinand that we did not know before—namely, that he was the dutiful end-product of an upbringing directed by his grimly Catholic Bavarian mother and the Jesuits.

The author's research appears to have been exclusively in published sources; his listing of secondary materials, extensive though not complete, seems to have played little role in his thinking. This may account for the air of superficiality that pervades the entire book. Characters, such as Gustavus Adolphus of Sweden, occasionally appear on the scene with no explanation of their motives. Even granting the limitations on his primary sources, Franzl might have drawn more from them than he did. For example, Ferdinand's lifelong problems with his younger brother, Archduke Leopold, who resented the secondary political status to which his birth consigned him, tell us much about the problems of seventeenth-century dynastic organization. They are not developed here beyond Leopold's role in the quarrel between Emperors Rudolph and Matthias over the Habsburg throne.

It may, however, be just as well that the book is no more heavily documented than it is. Franzl's system of footnoting is so cumbersome that it borders on the perverse. No footnote numbers are used; rather the reader is apparently supposed to notice catch phrases to which further references are attached in the endnotes. Sometimes quotation marks signal that documentation might be there, but

when these do not appear, one has to follow raw intuition. All of this involves a good deal of silly page-flipping; the practice should be discouraged whenever it appears.

PAULA SUTTER FICHTNER  
Brooklyn College,  
City University of New York

HANS MAGENSCHAB. *Josef II.: Revolutionär von Gottes Gnaden*. Graz: Verlag Styria. 1979. Pp. 300. DM 39.80.

The author of this study informs us that it is his intention to concentrate on the personality of Joseph II, to produce a psychological *cum* spiritual portrait of that complex ruler. He asserts that his book rests on exhaustive research in contemporary documents and in the literature. What he has produced, however, is an amateurish exercise in pseudo-Freudianism with occasional admixtures of wholly undigested structuralist theory. So far as this reviewer can discover, not a single unpublished source has been used by Hans Magenschab, and the sources he does use are, to put it kindly, exiguous. It would, in fact, be no feat at all to construct a wholly adequate working bibliography of Josephinism from the titles that he does not cite in his list of works consulted. (There are no footnotes.) Suffice it to point out that he does not use Mitrofanov, Arneth's *Maria Theresia*, or Valjavec.

The results of such autodidacticism are, predictably, uneven. Where Magenschab follows standard authorities, he is capable of describing an aspect of Joseph's rule succinctly and cogently, as he does with the attempted tax reform or with the catastrophic results of Joseph's Russian policy. More often, though, Magenschab goes ludicrously wrong. His treatments of Francis Stephen and of Maria Theresa are wholly one-sided. Joseph's father emerges as a bumptious and self-indulgent fool, his mother as a bigoted and reactionary virago. The assertion, made repeatedly, that Joseph was a true child of the Enlightenment who meant not only to enthrone philosophy but also to abolish the old order in favor of a bourgeois egalitarian society is just plain silly: whatever Joseph's innermost thoughts on the matter, it is evident that his reliance on bourgeois officials to carry out his reforms represents a means to an end, not an end in itself. Magenschab assumes that Joseph was a revolutionary spirit who occasionally made use of arbitrary methods because he believed they were necessary to ensure the survival of the Habsburg monarchy. In fact, there is good reason to believe that Joseph followed French and Prussian centralist models that placed humane values second to the proper functioning of the state.

As for the highly touted psychological insights,

the method used throughout is one of making a somewhat dubious assumption, then repeating it as if it were a given, and finally deriving conclusions from it. Thus, the author asserts that the relationship of Joseph's much-loved first wife, Isabella of Parma, to his sister Maria Christina was overtly lesbian and that this turned him into a life-long misogynist; that his ambiguous relationship to his father was deeply oedipal; that Joseph, who frequently quarrelled with his mother over matters of policy, actually wished her dead, thus saddling himself with life-long feelings of guilt; and that he was basically a masochist who was always bringing about the failure of his own projects in order to punish himself. Apart from the shaky logical ground that he occupies, Magenschab in arguing these propositions is continually falling prey to the anachronism that the "superior psychological insights that we have achieved" are necessarily valid for eighteenth-century relationships.

Having said all this, it remains to observe that the book abounds in factual errors, of which the description of Joseph in the last year of his life as "an old man of fifty-nine" (p. 56) is merely the most striking.

PAUL P. BERNARD  
University of Illinois,  
Urbana-Champaign

KARL VOCELKA. *Verfassung oder Konkordat?: Der publizistische und politische Kampf der österreichischen Liberalen um die Religionsgesetze des Jahres 1868*. (Studien zur Geschichte der Österreichisch-Ungarischen Monarchie, number 17; Schriften des DDr. Franz Josef Mayer-Gunthof-Fonds, number 12.) Vienna: Verlag der Österreichischen Akademie der Wissenschaften. 1978. Pp. 238. DM 50.

Seminars led by Adam Wandruszka on liberalism and the Catholic Church first inspired the author, a specialist in Austrian cultural history in the early modern period and Ottoman-Austrian relations, to begin work on the subject of this monograph. Using parliamentary reports, newspapers, brochures, and other printed materials, Karl Vocelka discusses the implications of the debates over the Religion Laws of 1868 for the German Liberal Party and constitutional government in the Austrian half of the Empire. These laws, moderate rather than radical reforms, were the result of Liberal efforts to reduce the influence of the Catholic Church. The Concordat of 1855, which had accorded the Church powers and privileges forbidden it since the time of Joseph II, was anathema to the Liberals' conception of a constitutional state. However, in 1868 the Liberals succeeded only in breaking the Church's hold on education, enacting a mild civil marriage law, and declaring the equality of all religions.

The debates over the promulgation and passage of the Religion Laws raged simultaneously in Parliament and in the press. Since the Liberal Party had neither a fixed philosophy nor unified adherents, the press played a vital role in formulating a Liberal position. Liberal failure to unite even on issues like the revocation of the Concordat that, as anticlericals, they generally supported, was a major weakness. Another was their lack of a popular hero. They did win the allegiance of an intrepid and vital press, which supplied enthusiasm for the movement as a whole.

Vocelka devotes the major part of the book to an assessment of the statements of Liberal politicians and journalists. He illustrates his points with copious documentation. Others have written extensively on the 1855 Concordat's impact on Church-state relations and on the unique relationship between the Liberal Party and the press. Vocelka acknowledges this in an extensive review of the literature in the preface. But he is the first to attempt a thorough examination of both the political and journalistic environment of 1868.

Vocelka sees the protracted debates over the passage of the laws as indicative of the Liberals' dwindling political viability. First, the proponent of revocation, Eugen von Mühlfeld, was defeated by the moderates. Second, press exhortations to pass a stricter measure were ignored in Parliament. Third, a miniature *Kulturkampf*, staged primarily by satirical journals, failed to strengthen the Liberal position. In the long run, the Liberals were unable to profit from the whole affair. Vocelka believes this to be a direct result of Liberal refusal to draw on their radical heritage of 1848. Only the school law, which removed primary schools from Church control, proved to be of lasting importance for the Liberal Party and for Austria.

While Vocelka illustrates that the Liberal weakness was already visible in what contemporaries regarded as a Liberal victory, he demonstrates less clearly the impact of the press on public opinion. His chapter on satirical magazines is primarily a selection of persiflages. In general, he more closely analyzes the press expectations of the Liberals than journalistic influence on public opinion.

This monograph provides a readable, tightly organized analysis of a number of complex issues. Vocelka develops his argument chapter by chapter and then thoughtfully summarizes it point for point in the conclusion.

MARY L. WAGENER  
Wilmington College

J. ROBERT WEGS. *Die österreichische Kriegswirtschaft, 1914-1918*. German version by HEINRICH MEJZLIK. Vienna: Verlag A. Schendl. 1979. Pp. 194. DM 46.



The book under review is narrower in scope than suggested by the title, for the treatment of the Austrian war economy is limited to certain industries: metallurgy, fuels, transportation, and armaments. A special chapter is devoted to manpower. Excluded from treatment are consumer goods industries, such as food and textiles, even though J. Robert Wegs points out that lack of food and clothing contributed to the Austrian defeat on the battlefield. In any case, food procurement should not have been omitted, had the aim of the author been to provide a comprehensive account of the war economy.

Be that as it may, the armaments-related industries that stood under direct control of the War Ministry are treated with great thoroughness in the carefully documented text. Actually, several government agencies, including the Commerce Ministry, regulated output and distribution, but their powers, unlike those of the War Ministry, were limited to the respective halves of the Dual Monarchy. An entire chapter provides a good overview of institutions involved.

The performance of war-oriented industries receives better marks than has been hitherto the case. Early Austrian misfortunes on the Russian front and those of 1918 on the Italian front are attributed by the author to poor strategy rather than to a lack of armaments comparable to that of the adversaries. He argues that even though Austria started the war relatively unprepared, it adapted remarkably well to war requirements. In steel and related industries the largest firms were given support, while smaller ones were allowed to close. Since allocation of raw materials to different uses followed a priority scale, many firms in the low-priority sector were forced to close as well. The labor thus released found employment in the priority sector. In addition to generating internal economies, concentration of production provided relief to the strained transportation capacities.

Metals and other commodities were distributed through specially organized "centrals," which were privately owned but stood under governmental control. Copper, which was in critically short supply, was recovered from boilers, electrical wiring, and church bells made out of bronze. Substitutes affected efficiency, for example, in transportation, since steel boilers in locomotives generated less pressure than their copper counterparts. War-related production reached its peak in 1916. From then on, bottlenecks in material inputs and transportation began to affect unfavorably both the quantity and quality of output.

Productivity declined steadily because skilled workers inducted into military service were subsequently replaced by less qualified labor. Lack of food and exhaustion through overtime work caused

additional decreases in productivity. Wages lagged significantly behind price increases. Nominal wages of inducted workers who continued in civilian work (*Landsturmpflichtige*) were lowered to the level of those in military service (*Heeresdienstpflichtige*). Relatively weak unions behaved in a docile manner, but military grievance commissions provided some relief from employers refusing wage adjustments, arguing that this would affect unfavorably their postwar competitiveness.

Investment in armaments industries was subsidized by the War Ministry within the framework of the armaments-oriented Hindenburg Program, to which Austria had acceded in 1916. One of the main beneficiaries was the gun manufacturing Škoda concern, which supplied 30.5 cm mortars even to the German army. Machine gun production was increased from 1,200 units in 1914 to 15,000 units in 1917. On the Italian front, machine guns were allocated to sectors where sharpshooters had been thinned out in combat—a chilling example of the substitution of labor with capital.

All in all, this work by Wegs is a welcome addition to the largely politically and militarily oriented literature on World War I.

TOUSSAINT HOČEVAR  
University of New Orleans

MANFRIED RAUCHENSTEINER. *Der Sonderfall: Die Besatzungszeit in Österreich 1945 bis 1955*. Graz: Verlag Styria. 1979. Pp. 416. DM 68.

Manfried Rauchensteiner, a younger historian of the University of Vienna who already has a work on military history in 1945 to his credit, has written a competent, informative study of the Austrian occupation, 1945–55. He places a heavy emphasis on the years up to 1950 and a somewhat diminishing one on the second half of this decade. The study rests on Austrian sources and British and American archival materials and is focused both on the policies of the occupying powers and the relations of the Vienna government with them. Austrian postwar domestic politics also attract the author's attention. On the whole a well-written and interesting account, it reaches conclusions that, though not startling, are unlikely to be radically altered by future research. The rather obvious fact that Austria paid with the occupation for its deep involvement with the Third Reich, its strong *grossdeutsch* and later Nazi orientation, its early enthusiastic endorsement of the Anschluss, and its role during World War II when Austrian soldiers fought loyally alongside the Germans should perhaps have been spelled out more clearly. This is not to say that Rauchensteiner has not attained a respectable level of objectivity;



but confining attention to the post-1945 period minimizes Austria's own past errors and magnifies the political and tactical mistakes of the occupiers regarding goals and methods.

In the Moscow Declaration of 1943 the Allies asserted that the treatment of Austria would be contingent on the contribution to its own liberation. This is another question that is not met head-on by the author; he rather implies (p. 197) that the annihilation of 65,000 Austrian Jews, carried out by German and Austrian Nazis, should have been calculated as Austria's sacrifices, as a "contribution" to its own liberation. In the immediate postwar era Great Britain assumed that Austria in its entirety would become a sphere of British influence. But since 1944 the Soviet Union had shown increasing interest in Central Europe. It did not, however, include Austria into the pro-Soviet, Communist-dominated belt of states, but pushed toward a quadripartite division of Austria like that established in Germany. Soon American political and military leaders began to look upon Austria as being of the "greatest political and military interest" and were convinced that the United States could not afford to let this key area slip into the Soviet fold, since this would lead to a weakening of America's strategic position in Germany and Italy. Such overriding American interests aside, the United States often played the role of mediator between the USSR and Great Britain, frequently arrayed against each other, while the Austrian government pursued a middle course, despite its pro-Western orientation in regard to domestic politics and ideology.

Soviet policy toward Austria was from the beginning shaped by the desire to provide through the occupation of eastern and northeastern Austria a shield for the protection of the developing Communist regimes in Hungary and Czechoslovakia (p. 63). In Austria, in contrast to the rest of East Central Europe, the Soviets restrained their ambitions and goals. Austria, having escaped the imposition of a Soviet-type government, was, according to the author, a unique case (*Sonderfall*). He does not always make clear the essence of this uniqueness and whether it should be credited to Soviet generosity, Austrian political skill, the occupation by several armies, the numerical weakness of the Austrian Communist Party, or the geographic distance from the USSR. Nor is Austria compared with the Communist states of East Central Europe. Since in the end the Soviet Union, for whatever reason, treated Austria better than most of its immediate neighbors, the author appears overly charitable toward the USSR and indulges in a sort of even-handed critical treatment of the occupying powers, pursuing a policy of "neutrality" similar to one to

which Austria itself, in accordance with the State Treaty of 1955, was committed.

ALFRED D. LOW  
Marquette University

WALTER POLLAK. *Sozialismus in Österreich: Von der Donaumonarchie bis zur Ära Kreisky*. Vienna: Econ Verlag. 1979. Pp. 319. DM 36.

Where social democracy failed in the first Austrian republic, it succeeded in the second. From 1945 to 1966 the Socialists participated in a coalition regime with the somewhat conservative Austrian People's Party (ÖVP); since 1970 they have governed alone, under the leadership of Bruno Kreisky, winning an absolute majority again in the elections of 1979. Socialism succeeded after World War II, contends Walter Pollak, because of changes in the social structure of Austria, as well as historic developments within the Socialist Party itself.

Austrian socialism from its beginnings in the 1860s grew in tension between revolution and reform, between radical social criticism and participation in state power. Victor Adler united moderate and radical factions to build a party that led a strong trade-union movement, expanded a network of cultural and welfare institutions, and nurtured the brilliant Austro-Marxist school of social science. When the multinational empire fell apart, Socialists took the main initiative in forming a republic. Averting leftist putsches, they were nudged out of power by the right in 1920. Austrian Socialism rejected both Bolshevik revolutionism and German socialist revisionism, but its radical phraseology isolated it and contributed to an increasing political polarization in the 1920s, with the Christian Socialists moving closer to the right and the emergence of fascist and Nazi groupings. Pollak scathingly castigates the party of Otto Bauer for ideological dogmatism, political illusions, and indecisiveness. The Socialist Party remained in impotent opposition, refusing opportunities to share the responsibilities of power or to organize a workers' uprising in the face of the right-wing threat that forced it underground and into exile in 1934.

Out of the rubble of the Second World War, the Socialists were able to rebuild their party and trade unions, but they acquired additional bases of support as well. Though the population of Vienna had declined from the peak of imperial glory, the smaller provincial cities had expanded, drawing country folk to manufacturing and service industries and into the Socialist Party in their new workplaces. In 1946-47 Austria nationalized much of its heavy industry and banking, not from Socialist pressure but to keep it out of the hand of the Soviet

occupiers. These nationalized industries, as well as the continually expanding institutions of the welfare state, formed a new and powerful social base for the Socialist Party, which is no longer narrowly ideological but considers itself (and is regarded by technocrats, officials, and managers) to be an energetic modernizing force in Austria.

Pollak contrasts Bruno Kreisky's leadership most favorably with that of Otto Bauer. This leadership is challenged by younger leftists who accuse Austrian Socialism of acting as management for modern international corporate capitalism and by environmentalists who succeeded in maneuvering a "no" vote on a nuclear power plant. Pollak insists, however, that socialist reforms within the system will lead to a radical transformation of that system—the position of Victor Adler.

An Austrian Socialist journalist, Pollak died in 1977; his book was edited with an afterword by his son, Michael Pollak. The volume is not a scholarly work and takes little account of recent historical literature. There are few new insights on the period before 1945; the author should have devoted more space to the Kreisky era, to data on the changes in Austrian social structure, and to the international context of Austrian politics. Surprisingly, little attention is given to Karl Renner; and the careers of the revolutionary socialists of the 1930s, such as Joseph Buttinger, are not followed into the postwar period. But the book is refreshingly unsentimental and presents a journalistic narrative, a "modern" history of a "modern" party.

INGRUN LAFLEUR  
Grand Valley State Colleges

ANNE DENIS. *Charles VIII et les Italiens: Histoire et mythe*. (Travaux d'Humanisme et Renaissance, number 167.) Geneva: Librairie Droz. 1979. Pp. 185.

The French invasion of Italy in 1494–95 has a different status in the historiography of the two countries involved: the French often date the beginning of their Renaissance from this first great encounter with Italian culture; the Italians look back on it as the beginning of a time of misery caused by foreign intervention. This work is concerned with the Italian side of the story, as it can be reconstructed from the large body of literature the event gave birth to: poems written on the occasion of entries, chronicle reports, and histories. Anne Denis has striven to extract and analyze the mythic elements that were operative in the midst of the historical act.

The chief myth, expressed mostly during Charles's trek south through Florence and Rome to Naples, was that the French king was to be a savior or liberator. The Medici-made equilibrium in Italy

was crumbling, to be sure, but that hardly explains the extravagant heralding of Charles VIII as another Charlemagne. Such rhetoric is best explained as typical humanist invention for the pomp and pageantry that characterized the march south. The principalities resisted only during the trip north; Charles barely escaped back into France with all his "conquests" lost. The French king was "the catalyst of a kind of nationalism" (p. 109) in the form of the Holy League created to harass him during the return in 1495; but, since Spain and the Empire were partners in the league, it is also true that Charles was the catalyst of a foreign interventionism that proved to be more important historically.

The Italians' disenchantment with Charles was rapid and thorough. They were used to the prince who selected himself by dint of *virtù* and so were quite dismayed by the personal mediocrity of Charles VIII—an average product of Salic dynastic roulette. They judged him to have been an instrument of *fortuna*—bad fortune in this case, since his advent marked the beginning of Italy's descent through a ring of woes down to the abysmal sack of Rome in 1527.

No great revelations emerge from this work. Indeed, the exhaustiveness of the research will appear to amateurs of the subject to yield only thematic repetitiousness. Obversely, however, those learned in the subject should savor the nuances of difference that can come only from detailed presentation of evidence. It is worthwhile, in any event, to have illuminated so well the state of mind of the citizenry in the most fervidly political part of the fifteenth-century world.

RALPH E. GIESEY  
University of Iowa

MARTIN LOWRY. *The World of Aldus Manutius: Business and Scholarship in Renaissance Venice*. Ithaca, N.Y.: Cornell University Press. 1979. Pp. 350. \$34.50.

As both its title and subtitle indicate, this book explores a range of topics in later fifteenth- and early sixteenth-century Venetian history and the history of printing through the career of Aldus Manutius. Manutius is generally known only as a printer who made widely available relatively cheap and convenient texts of the classics, especially in Greek, and who employed, among others, Erasmus. But, as Martin Lowry demonstrates, he was even more interesting than this might suggest. He was, for example, a respected scholar in his own right, who chose to become a printer only in middle age for reasons not entirely clear; Lowry argues that he did so out of a conviction that literary education improves character and hence that increasing the sup-

ply of the right sort of books would make the world a better place. This work is, then, among other things, a contribution to the history of humanism and its educational ideal.

But, once having entered the risky world of business, Manutius, to survive, had to confront a multitude of problems of a quite different kind; the nature of these problems and his solutions to them occupy some of the most vivid pages of this book. Clearly he had to compromise; to support his program of publishing Greek texts, he needed the profits from the more popular Latin classics and such pious works as the letters of St. Catherine of Siena. Among Lowry's contributions to our knowledge of the Aldine Press is the revelation that Manutius provided only a small fraction of its capital, a circumstance that further limited his publication of the scholarly editions that presumably interested him more than his partners. In fact, scholars were still divided about the desirability of extending learning to a wider audience, and one of the most interesting sections of this study demonstrates the uncertainty of the educated about the value of the printing press. One element in the significance of Manutius was, therefore, that he was in a position to mediate between scholars and printers. And his achievement was substantial, as Lowry reveals in his detailed survey of Manutius's production, year-by-year, from his beginnings as a printer until his death in 1515.

This book brings together every shred of evidence about Manutius, and it has important things to say about various aspects of his period. These two dimensions of Lowry's work are, however, not always very artistically combined, and one sometimes has the impression that, because the strictly biographical evidence was thin, some of the material about the times was added to fill in gaps. The result is a useful but occasionally unfocused study.

WILLIAM J. BOUWSMA  
University of California,  
Berkeley

ALISON BROWN. *Bartolomeo Scala, 1430-1497, Chancellor of Florence: The Humanist as Bureaucrat*. Princeton: Princeton University Press. 1979. Pp. xi, 366. \$25.00.

In this excellent study of the career, place in society, and writings of Bartolomeo Scala, Florentine chancellor from 1465 to 1497, Alison Brown has mastered the methodological problems of researching an individual who wrote and supervised the writing of hundreds of documents and who stirred antagonisms as a client of the Medici. She solves the first problem by focusing on his reform of the chancery

as well as its instruments and the second by avoiding the temptation to assess his significance in a simple republican and Medicean dichotomy. To explain Scala's search for office and favor, Brown joins the growing number of scholars who view patronage as a customary means of self-advancement in *quattrocento* Florence. Criticisms of Scala by his contemporaries imply, however, violation of standards of obsequiousness; these standards and the degree to which Scala conformed to them have yet to be determined.

The question of Scala's favor mongering should not obscure Brown's more important research on Scala's reform of the Florentine Chancery and integration of humanistic culture with a public life. Under Lorenzo de' Medici the Florentine government, having subordinated medieval corporations, achieved a sovereign power that required a reformed chancery to express its authority. Telescoping changes in government and chancery that other scholars have seen as occurring over the previous hundred years, Brown views Scala's introduction of vellum for state letters, his simplified salutations in state letters, and the institution of a group of secretaries who had full responsibility for administering foreign affairs as fundamental changes "from medieval chancery to modern secretariat" (p. 161). These secretaries inscribed documents and letters as servants of the sovereign authority of the Florentine people rather than as notaries whose authority derived from the emperor. The author recognizes that not all of these reforms remained in force but sees Machiavelli's appointment to the chancery without notarial status as an affirmation of Scala's labor.

Scala should be judged as a man of public affairs with a broad knowledge of humanistic culture, Brown contends, and not as a scholar. The author accepts Scala's explanation that his "trivial ability" resulted from a paucity of books and teachers in Vespasiano's Florence and his failure to study logic. The humanists had, however, explicitly eliminated logic from the *studia humanitatis*. Humanism informs his writings, which constitute "important and original reinterpretations of classical ideas with contemporary relevance" (p. 277).

Though eclectic, Scala constructed a coherent philosophy that resembles stoicism in its reliance on human reason, a reason most fully expressed in a single man. Human experience, being too dynamic to be encompassed by laws, requires one judge who comprehends the exigencies of time and circumstance and renders a justice fit for each individual. Brown recognizes that *quattrocento* Florentines understood this as legitimation of Medici rule, but she also views it as an attempt to come to terms with the humanists' enhanced perception of flux in

history. Concerning this and Scala's relationship to even broader interpretations of humanism, Brown will doubtless have more to say in her forthcoming edition of Scala's writings.

JAMES R. BANKER  
North Carolina State University

DAVID R. COFFIN. *The Villa in the Life of Renaissance Rome*. (Princeton Monographs in Art and Archaeology, number 34.) Princeton: Princeton University Press, for the Department of Art and Archaeology, Princeton University. 1979. Pp. xx, 385. \$45.00.

Twenty years ago David R. Coffin published a substantial monograph on the Villa d'Este at Tivoli, and since then he and his graduate students at Princeton have carefully re-examined a number of other important late sixteenth-century villas in and around Rome. The book under review, therefore, should represent an authoritative summation of a wealth of original research and—from the title—embody a much-needed synthetic study of architectural and social history.

The volume comprises nine chapters, the first two of which deal with the concept of *villeggiatura* as it was defined by early Renaissance humanists in Florence and as it was subsequently developed and practiced by the papal court at Rome, beginning with Paul II's *giardino segreto* at the Palazzo Venezia and culminating in a series of late *cinquecento* papal and cardinalate villas clustered about the hillside town of Frascati. As a *tour d'horizon*, these chapters are effective; his thorough knowledge of secondary literature and especially his exhaustive reading of unpublished *avvisi* preserved in the Vatican Library enable Coffin to document successfully the restless movements of Church dignitaries in search of rustic tranquility, healthier air, and lavish private entertainments during the months of *vacationes generales*.

Coffin then describes major monuments in three main groups: the early suburban villas, represented by Innocent VIII's Belvedere, Agostino Chigi's Farnesina, and the papal hunting lodge of La Magliana; then the more elaborate "ceremonial" retreats, including the Villa Giulia and the *vigne* of cardinals Caraffa, Grimani, Pio, and d'Este on the Quirinal as well as the Villa Medici and its neighbors on the Pincian Hill; and finally the classicizing examples of the Villa Madama, the Villa Lante, and the Casino of Pius IV. To these familiar works, Coffin adroitly adds a large assortment of relatively obscure *vigne*, *barchi*, and *casini*, so that nearly fifty individual buildings are discussed. In the final chapter the Farnese Palace at Caprarola, the Villa d'Este at Tivoli, and the Villa Gambara-Lante at Bagnaia are offered as the preeminent masterpieces of a golden age.

Unfortunately, the book fails to fulfill the promise of its title. Villa culture is presented as an essentially architectural phenomenon, and Coffin's approach to the subject depends almost exclusively on chronicle and description. Each building is laboriously reviewed as a unique artistic entity whose commission, authorship, building history, siting, design, painted decorations, and functions are dryly discussed in turn. As a result, the text resembles an extended catalogue, rich in new documentation but regrettably barren of comprehensive analysis. Issues of central concern to many architectural historians are never pursued. For example, Coffin believes that the origins of the villa are to be found in vernacular farm building, but he timidly avoids reconstructing a coherent historical evolution. Similarly, non-Roman precedents and developments are ignored, and the history of garden planning—surely of capital importance here—receives scant attention. Ideas about the villa from Renaissance architectural writers such as Francesco di Giorgio, Sebastiano Serlio, and Anton Francesco Doni are not invoked where they might have illuminated aspects of design or function.

To this unusually circumscribed view of villa architecture, Coffin brings an even narrower conception of its human context. The title's "life" is restricted solely to the pursuits of *otium* by the nobility. For this the *avvisi* supply newsy data concerning the whereabouts of important persons but little regarding the activities of tenants and caretakers, villa economy, or the pattern of social behavior inside the country villa. To be sure, these are difficult subjects, but they do require deeper research if the Italian Renaissance villa is to be comprehended beyond its intrinsic esthetic qualities.

This is the first book ever to appear on this topic in English, and it will certainly prove valuable to interested students as well as to specialist historians. Both its contributions and its omissions should encourage future studies in a remarkably attractive field.

RICHARD J. TUTTLE  
Tulane University

JOHN W. O'MALLEY. *Praise and Blame in Renaissance Rome: Rhetoric, Doctrine, and Reform in the Sacred Orators of the Papal Court, c. 1450–1521*. (Duke Monographs in Medieval and Renaissance Studies, number 3.) Durham, N.C.: Duke University Press. 1979. Pp. xii, 276. \$17.75.

Given the current interdisciplinary interest in Renaissance language theory and discursive practice, this monograph by John W. O'Malley has precisely the scale and range needed at this stage in the study of Renaissance eloquence. He has admirably ful-



filled his stated purpose: to investigate a "new body of evidence," the sermons preached before the pope from the pontificate of Nicholas V to that of Leo X ("new" because most of the texts were both unpublished and also unread by other scholars), by employing a "new hermeneutical device." He uses the canons and technical principles of the epideictic genre of classical rhetoric (the modes of "praise and blame") to reveal "changes in form, mood, and content" that will in turn illuminate the history of Renaissance preaching, "the theology and religious world view operative at the papal court," and certain issues in the history of Renaissance humanism as well (p. 3).

Of particular use to the historian is O'Malley's elucidation of the potential of genre studies. The study of genre lies at the point of intersection of social and intellectual historical inquiry; the study of "literary" genres is comparable to the study of "semiotic" genres, the codes and messages embedded in festivals, rituals, costume, or urban architecture. All genres are well-demarcated sites of very specific cultural activities. The study of genre evades the invidious choice between "form" and "content," or between social institution and intellectual theme, since a genre represents the institutionalization of a theme or themes and genre rules, governing the forms of address and reception, are particularly responsive to shifts in social needs, motives, and behavior.

O'Malley, to be sure, does not have the command of sophisticated techniques of discursive analysis that we find in French sociolinguists or even Anglo-American literary historians, but his erudition and common sense do not betray him. The chapter "A Renaissance Setting" is an admirable exposition of what the sermons were meant to do and the nature of the audience's expectations, expectations that turn out to be ambivalent, asking both erudition and brevity, elegance and piety. A forceful example of the contribution of genre study to intellectual history is his chapter on the ethical content of the sermons, "The Ideal of Christian Life"; O'Malley usefully undermines the rigorous opposition of traditional historiography between humanist and Scholastic intellectual elites in the Renaissance when he documents their separate reactions to the constraints of the sermon genre as resulting in the development of virtually identical ethical themes.

In sum, O'Malley uses the sermons to give a fresh perspective on Renaissance culture, illuminating such diverse issues as the extent of the classicizing or "pagan" bent of the Roman court, the culturally inclusive nature of the notions of reform within the papal circle, the limits of the pretensions to elegance and refinement of a particular Renaissance elite, and the shifts in didactic aims of the preachers,

which entails a revised view of the needs and motives of the homiletic audience.

NANCY S. STRUEVER

*Johns Hopkins University*

PAOLO SIMONCELLI. *Evangelismo italiano del Cinquecento: Questione religiosa e nicodemismo politico*. (Italia e Europa.) Rome: Istituto Storico Italiano per l'Eta Moderna e Contemporanea. 1979. Pp. xxxii, 490. L. 10,000.

In this weighty contribution to the much-discussed but still problematical issue of Italian Evangelism—now generally acknowledged to be philo- and almost always crypto-Protestantism rather than an indigenous current flowing into the so-called Catholic Reformation—Paolo Simoncelli applies more broadly the interpretive scheme set forth in his *Il caso Reginald Pole* (1977). To understand what Evangelism was and why its adherents failed to head off the rigors of the Counter Reformation, he insists that we must look at the interaction between its "internal" evolution (what went on in the minds, discussions, and writings of *spirituali* like Pole and Gasparo Contarini) and "external" developments (shifts in papal and imperial policy relating to the Protestant challenge, the exigencies of local political situations such as Cosimo I's effort to establish an absolute state in Tuscany) in the critical period from the mid-1530s to the mid-1550s.

The heart of this book (the introduction, chapters 1, 2, 4, and 5) is a detailed examination of these interrelationships. By about 1537, Simoncelli shows, three major Italian groups had emerged: radicals (including Antonio Brucioli, Marcantonio Flaminio, and Bernardino Ochino) committed to propagating openly in the vernacular the Protestant message of salvation by faith alone and predestination; moderates (Contarini, Gian Matteo Giberti, and others) personally convinced of these tenets but wary of the social consequences of disseminating them; and intransigents (above all Gian Pietro Caraffa, the future Paul IV) determined to crush the northern Protestants and silence their Italian sympathizers. During the 1540s the radicals set forth their position in words (notably in the *Beneficio di Cristo* of 1543) and action (the flight from Italy of Ochino, Pier Martire Vermigli, and Pier Paolo Vergerio), while the intransigents adroitly discredited the moderates by implicating Contarini (sent on a hopeless mission to the Regensburg Colloquy) in illegitimate compromise with Protestants. By the late 1540s the radicals were for the most part dead or departed and the moderates reduced to real or apparent (Nicodemite) conformity. Still, in the drastically reduced space left to them, the *spirituali* cleverly managed to express their con-



victions in veiled forms, such as incorporating close paraphrases of the banned *Beneficio* into their writings and utilizing literary genres (such as collections of vernacular letters) to memorialize their heroes and celebrate past moments of glory.

Simoncelli's main arguments are important and convincing. They would be easier to follow, however, if his sentences and paragraphs were shorter and if, adhering to J. H. Hexter's "maximum impact rule," he avoided excessive quotation, particularly from widely available published source collections. Two sections of this volume are somewhat peripheral. Chapter 3, a brief examination of hitherto neglected works and letters of Vittoria Colonna and others (texts included in appendixes), interrupts the presentation of his central argument: it should have been issued separately. Although the final chapter, dealing with "political Nicodemism" among Tuscan literary figures of various political stripes, helps to illustrate the major themes, it is admittedly a work in progress in which tentative hypotheses are being tested; hence its tone is not consonant with that of the book's core. Despite these shortcomings Simoncelli's work should be read with care by students of mid-*cinquecento* Italian religion, politics, and culture.

ANNE JACOBSON SCHUTTE  
Laurence University

ROBERT BONFIL. *The Rabbinate in Renaissance Italy*. Text in Hebrew. Jerusalem: Magnes Press. 1979. Pp. 327. \$20.00.

The rabbinate for Jews is the legitimate interpreter of their talmudic-legalist tradition that serves them as an authoritative guide in all areas of life and religion. Little wonder, then, that so many scholars are attracted nowadays to the history of the institution, trying to trace its development and fix its social and religious parameters. For this present study, Robert Bonfil has recourse to communal registers, for the most part in manuscript, to rabbinic *responsa*, legal formularies, homilies, and the like. Then he correlates all these data with information culled from municipal Italian archives. His principal aim is to determine the characteristics of the institution by placing it within the context of the socioeconomic system in which it developed. In this manner, he is able to explain in his first chapter the emergence of the rabbinic and Renaissance university. On pages 227–28, he even goes to the trouble of juxtaposing, column with column, a rabbinic ordination with a Latin one, to demonstrate to us how they adhere to the same literary pattern.

From chapter 2, we learn that some Renaissance rabbis did become public functionaries dependent upon the communal payroll. Mosse Basola (1450–

1560), himself a rabbi and an early explorer of Palestine, held all this in little esteem, claiming that such a relationship made the rabbis play into the hands of rich dignitaries (pp. 92–94). Bonfil, in one of the high points of his study, is able to compare the salaries of these appointed rabbis with those of contemporary workmen in the building trade (pp. 103–10). He concludes that the rabbis were poorly, indeed miserably, paid. No wonder that so many of the scholars searched for ways to make a living outside communal structures.

Some of those rabbinical Renaissance scholars, Bonfil suggests, deserve more attention in the future from researchers. Surely, we should have enough data to venture a biography of Jehiel Nissim of Pisa or of Mosse ben Abraham, Provençal (1503–75). This in turn might help us understand even better the way in which the civilization of the Renaissance reflected itself in the personalities of those rabbis (pp. 173–206). From an impressive collection of booklists, Bonfil is able to establish, by means of quantitative content analysis, that there was a clear decline in interest in scholastic philosophy, while the rising interest in mysticism should be understood as a kind of repentance practiced by the Jews who were marked by the trauma caused by the expulsion from Spain in 1492 (pp. 186–87).

The last part of the book consists of some sixty-four documents, many of which are published for the first time. Bonfil treats this part of his work with the same delicacy and care that he displays through the whole book.

JOSEPH SHATZMILLER  
University of Toronto

JEAN GEORGIN. *Venise au siècle des lumières*. (École des Hautes Études en Sciences Sociales, Centre de Recherches Historiques. Civilisations et Sociétés, number 41.) Paris: Mouton. 1978. Pp. 1,225.

The recipe for a popular history of eighteenth-century Venice, as Jean Georgelin points out, has been known for a long time. Start with a large portion of Casanova, add a measure of masked ball, *commedia dell'arte*, gambling, and scandal, finish off with Napoleon, and serve. Modern Venetian scholarship, for the most part, has overlooked the eighteenth century and has failed to penetrate very far beyond the superficial symptoms of urban senility. The exceptions are noteworthy: books by Davis and Tabacco on the decline of the nobility and its power to govern; important chapters by Lane and Fanfani on the late revival of trade; and the monographs of Beltrami and Berengo on population, agriculture, and social history. Georgelin's ambitious goal is a total history of eighteenth-century Venice, ranging from the traffic of the port and the output of main-

land industry to the political crisis culminating in the end of the republic itself. Cycles in agriculture, demography, prices, administration, crime, and public finance are explored, often by reference to massive statistical tables that unfortunately run the gamut from difficult to impossible to interpret.

Georgelin argues that the decline of the Republic of Venice was less the fate of its people than that of its ruling class and therefore its government. As such, the roots of the crisis extend back into an earlier age, with the formation of a government by exclusive closed caste. By the 1700s, governing power had fallen into the hands of about forty-two families. According to Georgelin, even though there was no great shortage of competent administrators, the oligarchy slowly ceased to rule in a progression of deepening crises.

Georgelin devotes a substantial part of his book to the fortunes of the patricians, their house property, mainland estates, wealth, and offices. Taxation, justice, and the bureaucratic structure of government also receive attention. But the story of how the government functioned, or failed to function, remains in large measure untold; the link between government and society is never fully forged. The reader does not know whether administration was efficient, whether government policy helped or hindered enterprise, and, perhaps most important, whether the crisis in government mattered at all to the livelihood of the people.

*Venise au siècle des lumières* may soon become the premier source book for eighteenth-century Venice but not its premier history. Georgelin's diligent mining of the archives has brought forth hundreds of tables and charts (the appendixes alone run over two hundred pages), but the narrative is often fragmented and inconclusive in its interpretation of the century's major economic, political, and social trends, almost as if consideration of these trends had been submerged by the massed findings of research on matters of lesser scale. Nevertheless, Georgelin's achievement in exploring the cyclic rhythms of this difficult century will endure as an important contribution to Venetian history.

RICHARD T. RAPP

*National Economic Research Associates*

LUCIANO GUERCI. *Condillac Storico: Storia e politica nel "Cours d'études pour l'instruction du Prince de Parme."* Milan: Riccardo Ricciardi Editore. 1978. Pp. xi, 477.

It has long been recognized that Enlightenment history often served the interests of Enlightenment polemics. The philosophes regarded the past as a battle waged between the forces of reason and ob-

scurantism. History's role was to elucidate this battle and ensure that the present generation adhered to the right side.

It is therefore not surprising that, upon his appointment as preceptor to the young Duke of Parma in 1758, Étienne Bonnot de Condillac composed a twelve-volume history as part of his charge's curriculum. Divided into the *Histoire ancienne* and the *Histoire moderne*, it is basically a pedagogic manual of interest to modern scholars because it illustrates how political ideals can determine the choice and treatment of historical events and themes.

Luciano Guerici's study is competent and scholarly. In the first part he places Condillac's pedagogic activities within the reforming milieu of mid-eighteenth-century Parma. In the second part he analyzes Condillac's treatment of specific subjects. The philosophe approves of those rulers and states that adhere to a nondespotic form of absolute rule, resist the temporal authority of the papacy, and prevent the pursuit of luxury from undermining their subjects' moral fiber. Those who fail in one or more of these categories are subject to criticism.

Given these criteria, the results are predictable. The balance sheet on Rome—even the Republic—is negative. Pursuit of wealth encouraged dissensions among classes, eventually causing political and social anarchy. Other people and events who fare badly include Gregory VII, the Crusades, and Louis XIV. Among those approved and recommended to the prince are Charlemagne, Louis IX, and Henry IV. Throughout the *Histoire* Condillac emphasized that rulers should moderate the use of their authority and should not allow the papacy to interfere in their states' temporal affairs.

Among this study's more interesting features is the examination in both text and notes of the way in which Condillac used and modified the historical opinions of such contemporaries as Voltaire, Mably, Millot, and Rousseau. In this context, the chapter on the Franks is of particular importance. Here Guerici links Condillac to the eighteenth-century debate concerning the origins of feudalism and the effect of the Frankish conquest on political institutions and social structure. Opposing the aristocratic interpretation of Boulainvilliers and the absolutist position of Mably, Condillac suggests that early Frankish rule was largely democratic and that absolutism developed for the most part after the reign of Clovis.

As is the case with many studies that attempt to relate ideas and environment, the connections between the two here tend to be vague. Parma's milieu may have provided the occasion for writing the *Histoire*, but its actual influence upon Condillac's historical thought is not clearly shown. Generally,

however, this book is a useful addition to the field of Enlightenment historiography.

EDWIN G. EHMKE  
University of Minnesota,  
Twin Cities

ANTON VAN DE SANDE. *La Curie Romaine au début de la restauration: Le problème de la continuité dans la politique de restauration du Saint-Siège en Italie, 1814-1817*. (Studien van het Nederlands Instituut te Rome, number 6.) The Hague: Staatsuitgeverij. 1979. Pp. 268.

The key problem that faced the restored papacy in 1814 was the need to adapt to a world transformed by revolution. To discover the extent to which this need was recognized at Rome, Anton van de Sande has analyzed the decision-making process of the curia on three questions: the reform of the Papal State, the ecclesiastical reorganization of Piedmont, and the negotiations with Vienna over Lombardy-Venetia.

Historians have usually depicted the curias as split into two hostile factions: *politicanti*, those prepared to make realistic adaptations to the post-revolutionary world, and *zelanti*, those dedicated to a rigorous defense of principle against opportunism, of tradition against adaptation. Sande finds this contrast too stark. The picture that he presents is of two vague and shifting groups, differing on tactics but in agreement on principles and basic objectives; the policies that emerged from their interaction were characterized by continuity with the eighteenth century.

Sande's interpretation seems valid where religious policy is concerned: here *politicanti* and *zelanti* alike were convinced that the primary objective was the establishment of effective papal supremacy over the church at the expense of bishops and rulers. As in the eighteenth century, therefore, the struggle against Jansenism and Josephinism, not the struggle against the revolution, was seen as crucial. No better example of this attitude—or of the tactical flexibility of the curia—can be found than the curia's determination to base the ecclesiastical settlement of northern Italy upon the Concordat of 1801, which, despite its revolutionary origins, had marked a notable advance for papal authority over the episcopate. In the negotiations with Piedmont, difficulties arose only when Victor Emmanuel wished to abolish this vestige of the French regime; the curia, however, insisted upon preserving its chief provisions and eventually won over the king with financial concessions. It met with more tenacious opposition in Lombardy-Venetia, where Francis I was resolved to introduce the Josephinist state-church, and negotiations ended in failure in 1817.

Sande's case is less impressive when he turns to

the Papal State. He argues for continuity between Consalvi's reform program and that of Pius VI, but this unduly minimizes the novelty of Consalvi's reforms, which went far beyond anything Pius VI had contemplated. Moreover, where reform was concerned, the most striking continuity in the curia lay not in a "tradition of *riformismo*" as Sande feels but rather in the consistent hostility of most of its members, which brought the defeat of every serious attempt at reform during the nineteenth century. Though Sande tries to minimize this hostility, it seems undeniable that here the traditional picture of a curia polarized between a reactionary *zelanti* majority and a *politicanti* minority is essentially correct.

Even the reader who does not fully accept Sande's arguments, however, will appreciate this perceptive work and the extensive archival research on which it is based; it is a welcome addition to the slim roster of recent studies on the Papal Restoration.

ALAN J. REINERMAN  
Boston College

MORRIS B. MARGOLIES. *Samuel David Luzzatto: Traditionalist Scholar*. New York: KTAV Publishing House. 1979. Pp. xv, 253. \$15.00.

Samuel David Luzzatto (1800-65), known as Shadal in Jewish history, was an Italian-Jewish scholar of second-order importance. There has been no intellectual biography of him until now. Morris B. Margolies provides this in a useful, though in turn quite middle-brow, fashion.

"Landscape and Influences" sketches the historical background. "Life and Work" concentrates on Luzzatto's tough life and on his many years as teacher at the Collegio Rabbinico in Padua. "Luzzatto's Religious World" outlines his rather simplistic ideology: he was committed to the divinely revealed character of the Pentateuch and to what he interestingly called the contrast between "Abrahamism and Atticism" (that is, ethics versus intellectualism). His scholarly work was concentrated on Biblical exegesis (grammar, history, and so forth). This obviously interests Margolies most, to the point that he occasionally even adds homilies of his own in the chapter "Exegesis." An extraordinarily large proportion of Luzzatto's writings was done in the form of letters, which he exchanged with his Jewish scholarly contemporaries (of whom several are more important than he); these were published either at the time or since. Margolies discusses them in "Luzzatto, Rapoport, and Other Contemporaries," and he adds a few previously unpublished, but also unimportant, letters in an appendix. Next to Biblical studies Luzzatto was chiefly interested in

Hebrew poetry, and he did important work in collecting and editing, especially the medieval R. Yehuda HaLevy's creations. He also published a lot of his own rather mediocre poetry in Hebrew, the language in which he did most of his work. A brief overall evaluation, bibliographies, and indexes conclude the book.

There was not much history in Luzzatto's life. He was a bookish man. He had a short fling of enthusiasm for the Italian revolution of 1848 (pp. 42-48), which one might have liked to see treated more fully. His Jewish "politics" consisted of devotion to the peoplehood of Israel in history and to a very moderate, essentially religious, proto-Zionism.

Jewish modernization occurred in Italy and Germany in the nineteenth century along parallel and, as Luzzatto's life shows, even parallax lines. His politics, as just outlined, coincided with those of his German contemporary, the founder of "neo-Orthodoxy," Samson Raphael Hirsch. The conservative wing of Jewish modernization shared heavily biblical—as contrasted to talmudic—Judaism, Hebraism, theological Zionism, and bourgeois liberalism. Most of Luzzatto's peers (and superiors) were Ashkenazic Jews. Even his "religious empiricism" is, in a simplistic way, a precursor of Franz Rosenzweig's highly sophisticated phenomenologism in the twentieth century.

Margolies is obviously not particularly interested in intellectual history. If he were, he could have made more even of Luzzatto's middle-brow conceptuality. (Some of the previous secondary literature treats him in such a perspective.) His view can be described as religious empiricism, equally opposed to the main Jewish-rationalist tradition and to kabbalistic mysticism. (This explains his strained relations with clearly the most important of his intellectual contemporaries, the philosophical historian, Nachman Krochmal.)

In a preachy foreword the chancellor of the Jewish Theological Seminary of America stakes a claim to Luzzatto in the name of American "Conservative Judaism." There is some ideological justification for this claim. On the other hand, Margolies mentions casually (p. 209) that Luzzatto's grandchildren, like Moses Mendelssohn's in Germany, were Christians.

STEVEN S. SCHWARZSCHILD  
Washington University

PAUL GINSBORG. *Daniele Manin and the Venetian Revolution of 1848-49*. New York: Cambridge University Press. 1979. Pp. xiv, 417.

CLARA M. LOVETT. *Giuseppe Ferrari and the Italian Revolution*. Chapel Hill: University of North Carolina Press. 1979. Pp. xiii, 278. \$15.00.

The Risorgimento, considered by some the triumph of nineteenth-century liberalism and nationalism, is one of the most studied periods of Italian history. Still, most studies of the Risorgimento seem to support the adage that in history, as in life, it is success that counts. Thus, those who provided the inspiration, political leadership, and the military means for the creation of the Unitary Kingdom have been studied and restudied while the role of the republicans and the radicals on the one hand—with the exception of Mazzini—and the figures of the Catholic counter-Risorgimento, on the other, have received far less attention. The volumes under consideration shed considerable light on the activity and thought of two men whose vision for Italy differed substantially from that of the liberal monarchists and moderates who molded the new state in their image.

Neither of these studies is, or pretends to be, a comprehensive life and times, so those planning such an enterprise for Manin or Ferrari may proceed with the assurance that they have not been pre-empted. As the titles indicate, Manin is studied in light of his role during the Venetian revolution of 1848-49, while Ferrari's thought and public life are examined within the context of the broader Italian revolution. Consequently the book by Paul Ginsborg is more of a monograph while the volume by Clara M. Lovett is a broader biographical survey. The latter work, however, concentrates on the protagonist's intellectual interests and public activity because the Ferrari archive of the *Museo del Risorgimento* yields little about his personal life. Nonetheless Lovett has made the most of the correspondence, comments, and glimpses of family, friends, and other contemporaries in her attempt to reconstruct Ferrari's private world.

Both Ginsborg and Lovett have tapped a wide range of primary sources, particularly archival, and Ginsborg provides a useful commentary on the many archives in which he has worked and the documents contained therein. The authors share a certain sympathy for the central figures in their studies and, in general, assess their contributions positively. Nonetheless, neither has permitted appreciation for the protagonist to cloud objectivity. Thus, although Ginsborg cites the decisiveness and superb leadership provided by Manin in the revolutionary events in Venetia in the spring and early summer of 1848, he does not hesitate to expose Manin's failure to forge an adequate political and military strategy for the preservation of the republic he was instrumental in creating. Likewise, Lovett reveals that, although politically Ferrari remained true to his radical orientation, philosophically he moved increasingly away from the belief in historical progress and the optimistic view of human nature that had nourished his political outlook. Lovett



does not delve into the anomaly of Ferrari's advocating the elimination of inherited fortunes to bring about political equality while enjoying a comfortable existence and pursuing an academic, literary, and political career made possible only by his own inheritance.

Ginsborg's detailed account of the making of the revolution in Venice and its fight for survival in the ensuing months is the first overall account since Vincenzo Marchesi's *Storia documentata della rivoluzione e della difesa di Venezia negli anni 1848-49* (1916) and G. M. Trevelyan's *Manin and the Venetian Revolution of 1848* (1923). Resting upon a broader documentary base than these earlier works, it avoids the hagiographic excesses of Marchesi and Trevelyan. In a clear and concise manner, if not always with literary flair, Ginsborg presents the events of the revolution within the social and economic setting of the Veneto in the nineteenth century as well as within the broader perspective of the European upheaval. Although the first two chapters provide a capsuled picture of Venetian society and chronicle the events leading to the outburst, the remaining chapters concentrate upon the revolution and Manin's role in it. The contents not only justify the title but also make an important contribution to the literature of the revolutions of 1848.

The Lovett volume, the first full-length study of Ferrari in English, provides insights both into the operation of the parliamentary left as well as the political process in Italy prior to the formation of mass parties. It makes available to a wide audience Ferrari's thoughts and writings as a radical intellectual, political theorist, and philosopher of history. Lovett maintains in her introduction that Ferrari was a political theorist no less original than Leroux, Proudhon, and Renan but notes in her conclusions that his socialism was derivative and remained vague while his arguments against laissez-faire liberalism were never developed into a coherent critique of bourgeois society. The aspect of his thought that she finds most original and significant for the history of the Italian left was his lifelong commitment to secularization embodied in his call for *irreligione*. Within the pages of this study in intellectual history, the author demonstrates that Ferrari played an important role in the international brotherhood of radicals that sought the secularization condemned by Pio Nono.

FRANK J. COPPA  
St. John's University,  
New York

VINCENZO G. PACIFICI. *Le elezioni nell'Italia unita: Assenteismo e astensionismo*. (Istituto di Storia Moderna, Università di Roma. Storia, number 6.) Rome: Edizioni dell'Ateneo e Bizzarri. 1979. Pp. 286 L. 9,000.

Although he acknowledges in full the import of the gap between *paese legale* and *paese reale* at the time of Italy's unification and the all too slow reduction of this gap by the extension of suffrage during the liberal era up to the eve of the First World War, Vincenzo Pacifici contends that the complexity of the problem cannot be appreciated without an examination of the twofold phenomena of electoral absenteeism and abstentionism: the former deriving from indifference, indolence, and logistic and climatological factors; the latter, an act of protest. In emphasizing the significance of absenteeism-abstentionism, and the gradual reduction of their incidence through the efforts of those very governing classes so much criticized for their slowness in closing the hiatus between the two *paesi*, Pacifici endeavors to demonstrate the liberal regime's capacity and determination to achieve a more perfect identity between the governing classes and the country as a whole.

Somewhat more than a third of Pacifici's work is devoted to an analytical summary of electoral proposals and legislation between 1848 and 1912, the theme of which is the gradual movement from an electoral system based on tax-paying qualifications to one based on minimal educational requirements. This movement is confirmed in the book's second chapter, which consists of an elaborate statistical national and regional study of the electoral body and its growth and composition, with an interesting aside on the extent of government intervention designed to influence the voters' choices. Only at the midpoint of his work—somewhat late—does Pacifici address himself systematically and consistently to the problem of absenteeism, which is examined in reference to such quantifiable factors as electoral constituencies that were slowly and imperfectly adjusted to population changes; the geographical area of constituencies; relevant transportation facilities; and voter turnout as influenced by weather conditions.

The incidence of Catholic abstentionism, inspired mainly by papal hostility toward a state created at the expense of the church's temporal prerogatives, is treated both in statistical terms and in the light of a profuse literature on the subject. Pacifici allots fewer pages to the briefer abstentionism of the groups on the extreme left, which were numerically less significant. A concluding chapter examines the incidence of absenteeism-abstentionism in twelve sample cities chosen for such qualities as historical antecedents, economic activity, topographic peculiarities, traditional devotion to the church, or marked preference for the left.

Although not the definitive study on the nature and significance of electoral absenteeism and abstentionism, Pacifici's work does much to deflate the notion that these phenomena were evidence of in-



herent and irremediable flaws in liberal Italy's political framework.

SALVATORE SALADINO  
Queens College,  
City University of New York

ALEXANDER J. DE GRAND. *Bottai e la cultura fascista*. Translated by PIETRO NEGRI. (Storia e Società.) Rome: Editori Laterza. 1978. Pp. vii, 299. L. 8,000.

Alexander J. De Grand's excellent study of Giuseppe Bottai deals with a highly intelligent and cultured Fascist, and one of its chief merits is that it accepts the credibility of Bottai's role as a Fascist *intellectuel engagé* without falling victim to the beguiling myth of Bottai the "good" Fascist. Fascism—like other modern political movements—had more than its share of faithful idealists who were dissatisfied with the realities of a regime they had helped to create.

De Grand's purpose was not to write a biography but rather to examine the nature of Bottai's ideas and aspirations, his efforts to implement them through the Fascist bureaucracy, and his overall impact on Fascist political culture. Based on Bottai's works and the periodicals he edited, as well as on public and private archives, the book offers a persuasive assessment of Bottai's thought by balancing his own internal vision with the external evidence of the sources.

A talented and elusive personality, Bottai (1895–1959) came to fascism in 1919 as a nationalist, an officer in the *Arditi* corps of World War I, and a futurist. He rose quickly in Mussolini's regime, serving from 1926 to 1932 as undersecretary and then as minister of corporations and from 1936 to 1943 as minister of national education. Bottai was bent on reconstituting the nation's "ruling class" in order to produce fundamental changes in the Italian economic, social, and political order. His original hope was to forge the Fascist party into the nucleus of a new, technocratic elite, but when he was blocked by Mussolini's statist policies he sought, unsuccessfully, to use the corporative system to achieve his ends. De Grand concludes that Bottai was essentially a conservative modernizer with technocratic tendencies.

Bottai's real importance was in his role as an intellectual catalyst. He maintained close ties with non-Fascist elements in Italian cultural life, vigorously defended modern currents in art against the reactionary tendencies within the regime, and worked desperately to stimulate and sustain an open dialogue of ideas and self-criticism within fascism. This effort to put himself at the center of an internal but loyal intellectual opposition attracted many of Italy's finest young writers and artists who, like Bottai himself, were becoming increasingly al-

ienated from Mussolini's dictatorship. His participation in the 1943 coup that unseated Mussolini reflected his belief that the regime had bankrupted itself but that its failure was one of leadership, not of system.

De Grand has produced a first-rate book, crisply composed, soundly researched, and skillfully structured. It will serve as a point of departure for future work on Fascist cultural policy, and, in its methodology and approach, it is a model study of the kind that is much needed in the history of Italian fascism.

PHILIP V. CANNISTRARO  
Florida State University

ROMAIN RAINERO. *La rivendicazione fascista sulla Tunisia*. Milan: Marzorati. 1978. Pp. 580. L. 10,000.

This prolific Italian historian has given proof of his ability to write critically on various issues of Italian colonialism, such as *L'anticolonialismo italiano da Assab e Adua* (1971), in which, besides using archival documentation, he complemented the research with accounts of personalities who participated in the political process. With *La rivendicazione fascista sulla Tunisia*, Romain Rainero's contribution is the exegesis of Fascist imperialism through the historical method; he reaches conclusions from a socialist point of view, hence the reason for incorporating the reaction of the political opposition to Fascism and the attitude of the Arab indigenous population. A large part of the volume is set aside to reproduce published and unpublished documents from 1868 to 1965. This is the ultimate work available to date on the Fascist claim to Tunisia.

The Fascist claim was facilitated by the presence of more than 100,000 Italians. Mussolini's demand for Tunisia on other occasions was based on French nonfulfillment of the Treaty of London of 1915. Mussolini took advantage of the territorial claim and turned it into a political showcase for internal use and a tool of diplomatic barter in the international arena. At one point in 1935, Mussolini was willing to abandon the protection of the Italians in Tunisia in exchange for concessions from Laval in Ethiopia. But in 1937–38, when France, after a change of government, refused to recognize the Italian Empire of East Africa, Mussolini threatened to repatriate all Italians from Tunisia although this would bring economic hardship to both the Italian community in North Africa and to Italy. Even more unrealistic was the Fascist hope to take advantage of Tunisian nationalism by supporting Arab political aspirations and allowing Bourguiba to broadcast against France from the radio station in Bari. It is difficult to understand how the Tunisians, who had seen Italian suppressive colonial policy at work

in Libya and Ethiopia, could have supported the Fascist cause in their homeland.

The implementation of Fascist policy in Tunisia was made possible by the Italian consulate and the activities of pseudocultural institutions. They coerced the Italian emigrants to follow the dictates of Rome, which maintained that all Italians residing in Tunisia were to be considered Italian citizens and should not be given French citizenship. Those Italians who were not affected by Fascist propaganda cooperated with Italian political refugees, who availed themselves of the press and mass media to accuse Fascism of preventing the Italians from deciding their own future within the French community.

Rainero shows, with empirical evidence, that the Italians in Tunisia became the political pawn of Mussolini's mercurial diplomacy; the Duce exploited them to further Italy's expansion in the Mediterranean. Rainero, together with other historians, is of the opinion that Mussolini's policy toward Tunisia was not consistent and can be explained in terms of opportunism and lack of organization. More specifically, the incoherence of Fascist foreign policy resulted from a policy of enhancing Fascist prestige before the Italians at home and confusing its enemies. Mussolini acted, vis-à-vis France, as a leader of a powerful nation that, in reality, was weak and divided within. To Mussolini's bluffs, France counterposed an attitude of patience and at times of intransigence, thus exposing the Duce's untenable policy and embarrassing him before the world.

*La rivendicazione fascista sulla Tunisia* makes for pleasant reading. The author's style is clear and to the point. Historians, students of colonialism, and persons interested in Fascism will find entertainment and pertinent information, and they will appreciate the well-balanced opinions and conclusions reached by the author. Rainero's work should also reach the Anglo-Saxon public through a translation of the book into English.

ALBERTO SBACCHI  
*Atlantic Union College*

C. M. WOODHOUSE. *The Struggle for Greece, 1941-1949*. Brooklyn Heights, N.Y.: Beekman/Esanu. 1979. Pp. xii, 324. \$29.95.

C. M. Woodhouse offers his readers as nearly unbiased an account of the internal military struggle in Greece between 1941 and 1949 as anyone is ever likely to be able to produce. He was himself a conspicuous actor in the first round between Communist and non-Communist Greeks when he headed the British mission in occupied Greece, 1943-44.

During this time he became well acquainted with many of the leading personalities in the struggle, and in subsequent years he followed events in Greece closely and continued to collect information from all sides. This book is therefore a record of Colonel Woodhouse's involvement in and understanding of Greek public affairs during the violent upheavals of 1941-49.

The genesis of the book imposes limitations as well as conferring the strength of authenticity. For example, Woodhouse has not poured over British or American archives, where details of official actions and policies are now available for historians to disinter from one-time secret files. He is, in fact, not much interested in the American role in Greece after 1947 and concentrates instead on the disagreements that plagued the Greek Communist Party leadership throughout the so-called third round. Since such disagreements were carefully hidden from outsiders at the time, this aspect of the book is indeed interesting and instructive. So far as I can tell, he is fair-minded in what he says, though his distaste for Nicholas Zacharaidis and his sympathy for Markos Vaphiades is obvious. But then, Zacharaidis does appear to have been an unwise as well as an unpleasant man; and Vaphiades was undoubtedly brave and at least comparatively straightforward in the policies he advocated.

A more important limitation of the book is this: Woodhouse remains content, for the most part, to recount what men said and did. He spends very little attention on the setting in which they acted—neither the international setting, nor the domestic Greek setting. When he does make remarks about Greek society, they appear as *obiter dicta* and do not carry much conviction, at least to this reviewer. Thus, for example, Woodhouse says of the year 1947: "The following year was to be one of military stalemate while that struggle [for the hearts and minds of the Greek people] was grimly fought out. It was not a struggle between classes, for all classes were equally represented on both sides" (p. 226). Yet a few pages later (p. 232) he points out that the democratic army failed to win recruits from the cities and had to depend almost entirely on men and women who lived in remote hill villages. It follows that there were significant class and geographic differences that distinguished the one side from the other. But Woodhouse has not thought much about these matters, and in the quoted passage, presumably, had a handful of leaders of the two camps mainly in mind.

But a history of words and deeds that is not tied into an analysis of the social setting within which men acted easily becomes a burden on the memory rather than an enlightenment of the mind. Woodhouse's book, for all its merits and admirable detachment, flirts with triviality for readers who did

not themselves live through those times or participate in the struggle he so meticulously chronicles.

WILLIAM H. MCNEILL  
University of Chicago

JERZY KOZEŃSKI. *Agresja na Jugosławię, 1941* [Aggression against Yugoslavia, 1941]. Summary in English. (Studium Niemcoznawcze Instytutu Zachodniego, number 32.) Poznań: Instytut Zachodni. 1979. Pp. 213. 55 Zł.

Jerzy Kozeński, a scholar at Poznań's Western Institute, specializes in the contemporary history of Eastern Europe. In this study, a work of diplomatic history, Kozeński describes the steps that led to the Nazi domination of southeastern Europe. He develops the topic along three major lines; the German-Italian confrontation and cooperation, Hitler's preparations for a war against the Soviet Union, and Berlin's policies toward Belgrade. The narrative takes a rather classic—one is tempted to say, dated—approach to the issues. History is made by personalities, foreign ministries, and diplomats. Economics, social forces, and political powers do not play a significant role in the scenario and sometimes are neglected altogether. Furthermore, the analysis of events preceding the military defeat of Yugoslavia by the Axis powers and the German invasion of the Soviet Union offers few new insights. A historian familiar with the region will derive only modest benefits from reading this slender volume.

Kozeński's sources are not unique, either; he uses mostly secondary and published primary material. Of the unpublished sources, the most noteworthy documents are those of the Central State Archives of the DDR and from the Slovak Central State Archives. No Yugoslav original papers were used. Wartime press and much of the contemporary emigrant literature—such as reminiscences of the Croatian Ustashi—were either overlooked or ignored.

Several significant details stand out in this book and justify its reading. Potsdam's files cast a new and intriguing light on the relations between Italy and the Third Reich; the story of the creation of the independent state of Croatia profits much from this provenance. (The slight attention paid to internal events in Yugoslavia and to radical movements like the Ustasha certainly reduces the value of the volume.) The not infrequent disagreements with postulates and clichés of Yugoslav historiography are interesting and rewarding. The disagreements include, among others, the evaluation of the political attitudes of Cvetković's government vis-à-vis Berlin (p. 74), the description of Maček's activity in April 1941 (pp. 127–35); the comments on the origin and role of the Chetnik-Mihailović resistance (pp. 163–66, and 174); and the discussion of the claim that

Yugoslav partisan warfare constituted the first "second front" in Europe (p. 168). The tenor of the writing is significantly non-Marxist, and the terminology and phraseology of historical materialism can hardly be detected.

The reviewed work is mainly a contribution *pro domo* to Polish historiography and to the Polish appreciation of the Second World War.

YESHAYAHU JELINEK  
The Balch Institute

HANS-JOACHIM HOPPE. *Bulgarien—Hitlers eigenwilliger Verbündeter: Eine Fallstudie zur nationalsozialistischen Südosteuropapolitik*. (Studien zur Zeitgeschichte, number 15.) Stuttgart: Deutsche Verlags-Anstalt. 1979. Pp. 309. DM 36.

This monograph is the fifteenth volume in a series of twentieth-century German internal and diplomatic history being published by Munich's *Institut für Zeitgeschichte*. Hans-Joachim Hoppe sees the significance of the German-Bulgarian relationship in the unique "independence" the Balkan kingdom exercised during the Second World War; hence the "eigenwilliger"—self-willed—of the title. Outstanding among Bulgaria's "self-willed" actions were the country's nonparticipation in the Jewish Holocaust—at least within the prewar boundaries—and its refusal to join the war against the Soviet Union with even a token force. Yet because a number of other "unique" relationships existed between the Third Reich and small European countries during the war, for example, Spain, Finland, Hungary, and Rumania, it would seem that, although Hoppe's points on Bulgaria's independence of action are partially valid, the value of his work lies in its description of Bulgarian-German relations rather than in revealing some universal key to German wartime diplomacy.

Hoppe has thoroughly exhausted relevant German archival material and pertinent secondary works in German, English, and Bulgarian. He has used some printed Bulgarian primary materials but has neglected other important accessible documents such as the published extracts of Prime Minister Bogdan Filov's diary, the minutes of the national assembly, and the official gazette. (Available Bulgarian archival material from this era, especially for use by Western scholars, is of course severely limited.) A few minor errors have carried over from the author's sources. None of these significantly damage the work, and only one needs to be called to attention. The barracks conspiracy of April 1942 was apparently not related to the espionage trial of General Vladimir Zaimov. Filov's diary makes this quite clear, although the German embassy reports confuse both matters. Hoppe, like Marshall Miller,

has continued this mistaken amalgamation. Both affairs are still rather mysterious. Surprisingly there has never been more than scant mention of the barracks uprising (allegedly led by Communists) in postwar Bulgarian historiography; and, although much has been written about war hero General Zaimov, the precise nature of his activity has not yet been fully revealed.

Much of Hoppe's material has been covered in recent English monographs and articles by Miller, Nissan Oren, and this reviewer, but this new book still is valuable even for English-speaking scholars. Hoppe's detailed accounts of German-Bulgarian diplomatic dealings both in the thirties and during the war are unmatched by the previous literature. Hoppe also spends significant and profitable effort in examining the important German-Bulgarian economic relationships. Scholars will find his appendix listing members of Bulgaria's governments, his detailed explanatory notes, and his extensive bibliography of interest as well. In short *Bulgarien—Hitlers eigenwilliger Verbündeter* is a welcome addition to the scholarly literature on twentieth-century Bulgaria.

FREDERICK B. CHARY

Indiana University Northwest

GERALD J. BOBANGO. *The Emergence of the Romanian National State*. (East European Monographs, number 58.) Boulder, Colo.: East European Quarterly; distributed by Columbia University Press. 1979. Pp. xiii, 307. \$17.00.

Gerald J. Bobango sets out his purposes as (1) to introduce Western readers to a critical period in nineteenth-century Rumanian history, drawing on an impressive array of Rumanian scholarly works that other Western historians have ignored; and (2) to illustrate the pattern of the development of nationalism in Eastern Europe and of liberation and nation-building in the Balkans. On the first count he has no need to apologize for the shortcomings of Western scholarship. A new generation of historians since that of the elder Seton-Watson and Thad Riker has moved into the field of nineteenth-century Rumania and made good use of the voluminous work of Rumanian scholars—who have flowered with the official cult of the national past, even though theirs remains a controlled profession. The dialogue between Rumanian and American historians has been a continuing phenomenon over the past decade. Nor has the author any need to apologize for his own work, which stands as a solid, original contribution and not merely a summary of the research of Rumanian scholars.

On the second count, the pattern of nationalism, Bobango does little more than try to see how Rumanian cultural and political developments fit the

general theories of nationalism worked out by Kohn, Snyder, and others and to make a rough comparison with other Balkan nations. The analysis is not carried very far, and indeed it is a little difficult to draw any profound conclusions from the very limited period the book covers, the seven confused and strife-torn years from Prince Alexandru Cuza's double election in the two Danubian Principalities in 1859 to his forced abdication in 1866, except perhaps that they illustrated the growing pains of the "emerging national state" (to borrow the wording of the title).

The book's great merit lies not in the realm of social theory but in its painstaking, detailed, and objective account of Cuza's reign. Building on the pioneer work of A. D. Xenopol and on C. C. Giurescu's recent biography, drawing also on source material from the Cuza archives and elsewhere, the author covers the main issues (merger of the two principalities, relations with outside powers and with "liberation movements," secularization of the Greek monasteries, the experiment in parliamentary government and its breakdown, the Bonapartist coup d'état, and the land reform) and covers them well. It is refreshing, too, that his judgments are bold and clear, not cloaked in cautious qualifications. For example, he is sympathetic to Cuza and his moderate colleagues who practised the art of the possible, especially Mihail Kogălniceanu, probably the only real statesman of the time. On the other hand the so-called radical-liberals, such as Ion Brătianu and C. A. Rosetti, earn no laurels. Those two were to play leading roles in Rumanian political life long after Cuza passed from the scene, but on the record of these years they come through as selfishly ambitious, hypocritical, and dishonest.

Because he has done his research so thoroughly, but avoided getting bogged down in it, Bobango is able to catch the spirit and tone of Rumanian politics, and in that sense much of what he writes has meaning beyond the short span of the years of Cuza's reign.

JOHN C. CAMPBELL

Cohasset, Massachusetts

EMILIA ȘONEA and GAVRILĂ ȘONEA. *Viața economică și politică a României, 1933–1938* [The Economic and Political Life of Rumania, 1933–38]. Summary in English. Bucharest: Editura științifică și enciclopedică. 1978. Pp. 330. 15.50 L.

MIHAIL RUSENESCU and IOAN SAIZU. *Viața politică în România, 1922–1928* [Political Life in Rumania, 1922–28]. Bucharest: Editura politică. 1979. Pp. 261. 11.75 L.

Here are two more much-needed monographs on early twentieth-century Rumania, based on essen-



tial published and unpublished material that has lain fallow since the end of World War II. The authors, especially Emilia and Gavrilă Șonea, have assiduously investigated a rich variety of sources in the *Arhivele Statului București*, the very important *Arhiva Institutului de studii istorice și socialpolitică de pe lângă Comitetului Central al Partidului Comunist Român*, and elsewhere. Unfortunately, the cryptic archival citations of the Șoneas, Mihai Rusenescu and Ioan Saizu's sloppy footnotes, and the lack of separate bibliographies lessen the potentially great usefulness of both volumes to the scholar.

This sort of inattention to the reader's viewpoint should not, however, obscure the very real contributions of both these works. *Viața politică în România, 1922-1928* is particularly valuable for its description of the political orientations and programs of the parties. Its authors document a large number of examples of election abuses and party maneuverings, but they emphasize, quite rightly, the process of consolidation and settling-down that characterized Rumanian political life in the mid-1920s.

By contrast, the Șoneas' subject is almost unbelievably grim. Although economic recovery did begin in the mid-1930s, their real story is the increasing demoralization of politicians and the disintegration of even the forms of parliamentary government. In spite of the changes in old parties and the proliferation of new groups and political theories, *Viața economică și politică a României, 1933-1938* contains little background on party beliefs, membership, or programs. It does, however, present considerable new detail on legislation, electoral manipulations, interparty negotiations, intraparty bickering (especially the quarrel between *bătrânii* and *ținerii* in the National Liberal Party), and on the royal camarilla's use of political instability to further King Carol's dictatorial plans.

All four of these authors share certain distinctive strengths, weaknesses, and attitudes toward historical writing. They shine in straightforward, well-documented narrative. Sometimes, however, their ideological stance is rather obtrusive. Both books, predictably, devote substantial chapters to the Communist Party, without comparable space being given to other small groups. The rather mechanical repetition of set phrases ("the deepening contradictions within the bourgeois-landlord class") have a deadening effect on style. Further, a common anti-Great Man orientation results in little information or evaluation in either book of such pivotal figures as Ionel Brătianu or the two kings. Statements of motive tend to be relegated to footnotes.

For the most part, however, these predispositions do not mar the authors' clearly genuine attempts at historical objectivity. Except for an excessively shrill and negative tone adopted by Rusenescu and Saizu

toward Iuliu Maniu, the judgments given on the work of "bourgeois" parties and governments are calm, balanced, and well supported. The authors are all well aware of the complexity of the issues.

My principal reservation about these books is related to neither their accuracy nor their objectivity, but rather to the curiously traditional nature of their approach. Ironically, while Western writers of Marxist views have enriched historical studies by taking into account previously neglected social and economic factors, many Rumanian political historians seem to belong to an earlier and narrower school, focusing exclusively on the political activities of a small elite of party leaders.

As a result, some very intriguing questions simply never are raised. For example, the authors of both these books merely assume the close connection between National Liberal politicians and the major economic enterprises in Rumania, rather than probing into it, on the lines, say, of the innovative work of Al. Gh. Savu (*Sistemul partidelor politice din România, 1919-1940* [1976] and *Dictatura regală* [1970]). Savu began to explore the ramifications of the political-economic network for the mid-1930s, but nothing of the sort has been done for the 1920s, and much more information still is needed on the 1930s. King Carol's political takeover would be much clarified by data on specifically how the Malaxa-Auschnitt-Blank group of industrialists around the king and Prime Minister Gheorghe Tătăreanu managed to breach the old Brătianu liberal economic monopolies. The Șoneas' chapters on the economy are not sufficiently integrated with their political chapters. Although they allude to both domestic and foreign pressures from the right and, like Rusenescu and Saizu, are very critical of foreign economic penetration, the Șoneas nowhere consider the identity of the foreign investors or their political significance. This sort of imaginative analysis is sorely wanted.

Nonetheless, despite their shortcomings, these two books, because of their detailed presentation of many of the basic facts of the period, will surely be required reading from now on for anyone concerned with interwar Rumanian political life.

VICTORIA F. BROWN  
University of Washington

VÁCLAV ZÁČEK. *Josef Václav Frič*. (Odkazy Pokrokových Osobností Naši Minulosti.) Prague: Melantrich. 1979. Pp. 391. Kčs. 35.

Because the Czechs lack a distinct revolutionary tradition in modern history, post-1948 Czech historiography has understandably highlighted Czech individuals who were somehow connected with "progressive," democratic, and revolutionary move-



ments in Europe. Josef Václav Frič (1829–90), romantic poet-dramatist and revolutionary, was such an individual. The son of a Prague lawyer, Frič was a university student when the Czech revolution broke out in 1848. He joined the young Prague radicals, participated in the June Uprising, and helped organize the ill-fated May Conspiracy of 1849. His penalty for these patriotic activities was imprisonment and eventually exile to Transylvania. In 1859 Frič left Austria for what turned out to be a twenty-year odyssey, half of which was spent in promoting revolution against Austria and the other half in seeking repatriation with his family in Prague. Successful only in the latter, he lived out the last decade of his remarkable but tragic life on Czech soil.

Václav Žáček, the leading authority on nineteenth-century Czech-Slavic relations, has chosen to focus primarily on Frič's political and revolutionary activities of the 1860s, thus filling a significant gap in the literature on Frič. The value of this excellent monograph is enhanced by the fact that Žáček bases it partially on the first systematic study of the rich personal and family papers of Frič, which were made available to researchers only after World War II. Žáček painstakingly details Frič's relentless efforts to have the Czech question placed on the agenda of the European revolutionary movement. Frič made contact with Bakunin, Garibaldi, Herzen, and Kossuth, as well as with émigré German and Polish revolutionaries. With little success he attempted to tie Czech emancipation to the anti-Austrian Italian unification movement. When the Poles revolted in 1863, Frič, a lifelong Polonophile, urged the Czechs to support the rebellion in the hope that the Poles, once free, would help to liberate the Czechs. After the Polish debacle, he participated in a project to interest Garibaldi in an anti-Austrian coalition of Croats, Czechs, Hungarians, and Poles, which was never realized. In the Austro-Prussian War of 1866 Frič was involved in a plot to provoke a Czech uprising, in return for which Bismarck was to reward the Czechs with independence. However, the revolt never took place, and the defeat of Austria resulted in the Austro-Hungarian Ausgleich, which was extremely detrimental to the Czech cause.

Although Frič was certainly the single important Czech revolutionary figure in the nineteenth century and did much to acquaint Europeans with the plight of the Czechs, the prominent place in the history of modern Czech national and political life that Žáček assigns to Frič is questionable. Frič's repeated calls for Czech liberation from Austria met with scant support either from abroad or from the Czechs themselves. And, as Žáček points out, Frič's intense hatred of Austria and the Habsburgs led him to collaborate with some unreliable allies and to adopt at times unwise policies and tactics. A revolutionary without a party, Frič remained well

outside the mainstream of the Czech national movement, and his goal of Czech independence was left for a future generation to accomplish under much more favorable circumstances.

THOMAS D. MARZIK  
Saint Joseph's University,  
Philadelphia

JON BLOOMFIELD. *Passive Revolution: Politics and the Czechoslovak Working Class, 1945–1948*. New York: St. Martin's Press. 1979. Pp. 290. \$22.50.

Jon Bloomfield offers an interesting angle of approach to the ascendance of the Czechoslovak Communist Party to undivided power in the period 1945–48. His focus is on working-class support for the party's policies. With the help of a fairly detailed reading of existing literature, contemporary press, and such documentary evidence as can be marshaled, he is able to conclude quite confidently that the Czechoslovak working class "was a largely willing accomplice of the revolution, but not its driving force." This was "a passive revolution," to which the impetus "came from above and from abroad." As such, the revolution of 1945–48 "had enormous implications for democracy in the future socialist state," namely, political power did not pass fully into the hands of the workers but rather into those of the leadership of the Communist Party. For a professed Eurocommunist, such as Bloomfield, the aberration is evident. February 1948 in Czechoslovakia—the takeover—was neither the culmination of a masterful Communist strategy backed enthusiastically by the masses nor a simple coup d'état. What it was, he is less clear in saying.

Bloomfield's vantage point is not without merit. He can say and prove that the view of the Communist takeover as no more than the result of domestic and international power politics is much too simplistic. There *was* popular, above all workers', pressure for socialism and support for the Communists. The population was not wholly and intrinsically opposed to the plans that Gottwald and company laid out before them. In fact, the willingness with which large crowds assembled at the Communist leaders' beck and call, and even executed patently undemocratic and unlawful orders issued from Communist secretariats, casts doubt on Bloomfield's taxonomic usage. There was nothing "passive" about the street demonstrations, the general strike, the attacks on non-Communist party offices, the pressure on President Beneš, or the formation *en masse* of the so-called Action Committees and the Workers' Militia. Bloomfield may argue that large segments of the working class condoned the thrust of the activists while not steering the course of the revolution themselves. But then, a truly working-class revolution, self-managed, as it were, by the

toiling masses themselves, is a bookish phenomenon. In real life things simply do not happen that way.

To this reviewer it is anyway not the adjective but rather the noun that calls for clarification. The Communist road to power in Czechoslovakia was undoubtedly "active," not "passive." But was it a revolution? Or a coup? One might perhaps profitably distinguish between the process of transformation that occurred in Czechoslovakia from 1943 (Beneš's first trip to Moscow) to, say, 1949-50 (the consolidation of a Soviet-type system) and the actual events of 1947-48, especially the planning and execution of the February takeover. Whereas one would not hesitate to perceive of the six or seven years of societal transformation as revolutionary—they saw the institution of a new sociopolitical and economic order—the shorter conflict of 1947-48, central as it was to the long-term process, had many of the characteristics of a sharp and swift coup d'état. The one important exception was that the size of the group of its planners and executants exceeded the usual small numbers of dedicated putschists. Precisely because the February 1948 events had a wider context, both internationally and in terms of popular participation at home, their designation as a coup is inadequate. The concept of a *takeover*, in the sense of assumption of full, undivided control, would seem to possess the most appropriate descriptive quality.

All this taxonomic quibble would of course remain without much benefit should we lose sight of the content of our nominal devices. It pays to the historians of Communism to practise "realism," not "nominalism." Bloomfield's book should be read as a useful bringing together of a wealth of facts about the participation of the Czechoslovak workers—or the lack of it—in the process that led to the institution of a Communist order in their country. His conceptual framework need not be accepted: it has logical holes, and it makes the mistake of virtually all idealists in passing judgments in the name of a utopia, in this case a socialism that would be neither social-democratic nor Stalinist but somehow democratic and Marxist at once.

VLADIMIR V. KUSIN  
Radio Free Europe—Radio Liberty,  
Munich

TADEUSZ WYRWA. *La pensée politique polonaise à l'époque de l'humanisme et de la renaissance: (Un apport à la connaissance de l'Europe moderne)*. Preface by PIERRE CHAUNU. Paris: Librairie Polonaise or Poets and Painters Press, London. 1978. Pp. 684. 110 fr.

Anyone writing in a Western language about the *terra incognita* of medieval and early modern Poland is faced with the choice of either producing a con-

cise but narrow monograph that non-Polish readers will have difficulty relating to wider historical concerns or else attempting an all-encompassing treatment that supplies the broad narrative background with which those who do not read Polish cannot be familiar. Wisely—but at the price of asking the reader to go through a very hefty volume—Tadeusz Wyrwa has assumed the obligation of being comprehensive and comparative. He introduces his reader to many of the complexities of Polish history down to 1600, and his comparative sweep ranges from Spain to Russia. This is truly an ambitious book.

One hesitates to suggest that so formidable a study oversimplifies and lacks thoroughness; yet, regrettably, it does. Wyrwa overlooks important modern Polish and Western scholarship (major names absent from the bibliography include Brock, Plaza, Schramm, Sucheni-Grabowska, Weintraub, and Williams). Readers may find disturbing the constant resort to broad, sometimes tendentious or obsolete, abstractions, such as juxtaposing medieval to "modern" Renaissance ideas and portraying Polish-Russian conflicts as the confrontation between an aggressive despotism and a peaceloving *Rechtsstaat*, thus ignoring the Jagiellonians' and Batory's expansive policies. Seeing Polish preference for Calvinism over Lutheranism simply as an antipathy to German authoritarianism may be dated. Wyrwa might also have considered the great importance of Wittenberg and Königsberg universities for the Poles, the political and social outlook of Polish Calvinism, and the disillusionment that many *szlachta* felt toward Calvinist authoritarianism after 1560. Some of these matters could have been illuminated by the use of recently published synod protocols.

Wyrwa rightly emphasizes the importance of efforts at legal codification as a factor in the formation of *szlachta* political ideas during the sixteenth century. Much more, however, might have been done to relate legal reform to the Execution of the Laws movement, the protracted drive by the Sejm's lower chamber to effect restoration of alienated royal domains and restrain overmighty magnates and churchmen. The Execution movement was closely linked to *szlachta* appeals for royal support of the Reformation and union with Lithuania. Indeed, until 1572 almost all Polish political writings dealt with questions raised by the calls for Execution or Reformation. Contemporary West Europeans would have seen the Execution movement as an effort to defend "the police" of the commonwealth. Wyrwa conveys little of the sense of times out of joint that pervades Sejm protocols and the writings of the prolific gentry publicist Mikołaj Rej—sources unaccountably not exploited in this book.

Late medievalists and early modernists will nevertheless find much of value here. Wyrwa is good on a number of important figures—the conciliarists,

Jan Ostroróg, Stanisław Orzechowski, Jan Kochanowski, and especially the Erasmian political theorist Andrzej Frycz Modrzewski. Above all, this book should make Renaissance scholars more anxious to extend eastward the conventional geographical boundaries of their field.

JAMES MILLER  
Stockton State College

KAY LUNDGREEN-NIELSEN. *The Polish Problem at the Paris Peace Conference: A Study of the Policies of the Great Powers and the Poles, 1918-1919*. Translated by ALISON BORCH-JOHANSEN. (Odense University Studies in History and Social Sciences, number 59.) Odense, Denmark: Odense University Press. 1979. Pp. 603. 120 KR.

This is a very important study. Even though much has been written about the Polish settlement in Paris, Kay Lundgreen-Nielsen, a young Danish historian, has produced a highly original monograph on this topic. Covering roughly the period from the 1918 armistice to the signing of the Treaty of Versailles, this is the first book based on such exhaustive documentation drawn from British, American, French, and Polish archives and methodologically conceived along novel lines. Unlike other historians, Lundgreen-Nielsen did not confine his very detailed analysis to the policies of the great powers but examined extensively those on the Polish side. His claim that only a study of Allied-Polish interaction allows us to get a full picture is entirely vindicated.

The *Polish Problem* is not an easy book to read, and the English translation could have been better. Nor is the index very helpful. But these are minor flaws. The author has done his best to provide the reader with guideposts in the form of short conclusions at the end of each section. To the specialist, he has given a richness of documentation and insights that is most rewarding. The 150-odd pages of footnotes that follow some 430 pages of text are a mine of information. Lundgreen-Nielsen has thoroughly digested and mastered the material, and in presenting it displays a remarkable precision and a high degree of accuracy.

The author, however, has done much more than to put together, in a judicious and balanced way, a vast amount of material. His main achievement lies in drawing important conclusions and advancing new interpretations. Lundgreen-Nielsen has demonstrated, for instance, the existence of a broad variety of views within the British, American, and, to a lesser extent, French groups of experts, a fact thus far either unappreciated or glossed over. Thus, he could show the machinery of the process of making decisions by the Big Three. Concerning the Polish side, the author has rejected the prevalent view of a

*de facto* division of labor between Piłsudski and Dmowski, the former concentrating on winning frontiers in the east, the latter obtaining the western borders at the peace conference. Lundgreen-Nielsen shows convincingly that Piłsudski clearly perceived the connection between the settlements in the west and the east and was concerned with both. The thesis that the dualism in Polish foreign policy, of which the Allies were perfectly aware, damaged Poland's case is persuasive and perceptive. The stress on Dmowski-Paderewski differences is amply documented.

A short review cannot do justice to the many important points Lundgreen-Nielsen makes in his book. In view of the fact that neither he nor his publishers are Polish, the minimal number of linguistic errors is very much to their credit. The author's evenhandedness in the treatment of an exceedingly difficult topic deserves the highest praise. While I would hesitate to call any book the last word on any subject, I consider Lundgreen-Nielsen's work to be *the* book on the Polish problem at the Paris Peace Conference.

PIOTR S. WANDYDZ  
Yale University

SEWERYN AJZNER. *Związek Związków Zawodowych, 1931-1939* [The Union of Trade Unions, 1931-39]. Warsaw: Książka i Wiedza. 1979. Pp. 458. 100 Zł.

Polish historiography has been recently enriched by a number of pioneer studies allowing better understanding of the political-economic mosaic of the pre-1939 Poland. Seweryn Ajzner, a reputable Communist Party historian, devotes his latest monographic work to the Union of Trade Unions (ZZZ), a Polish variety of *Zubatovshchina* and a rather little-known chapter in Polish history.

The ZZZ was a powerful labor organization of the 1930s, established to channel trade union activities toward progovernment support, but that by 1939 became the loudest exponent of the most radical circles of the *Sanacja* left. Its origins were rooted in the 1926 split in the National Workers' Party (NPR) and Józef Piłsudski's influence in the Polish labor movement.

By 1930 influential circles within the Piłsudski government came up with an idea to unite all Piłsudskian trade unions and labor organizations. It was hoped that the new organizational body, by cleansing its members from any party influences and creating a formidable challenge to the Polish Socialist Party (PPS), could consolidate the labor movement under the banner of cooperation with the *Sanacja* camp.

This task was entrusted to Jędrzej Moraczewski, a former member of the Piłsudski cabinet and a former member of the PPS. In May 1931 the diffi-

cult job was completed, and the newly created ZZZ with its one hundred and twenty to one hundred and thirty thousand members became the third largest labor union organization in Poland. It was comprised of some thirty trade unions nationwide.

The honeymoon did not last long, however. By 1933 a strong rift developed between the ZZZ and the government. The main conflict was over the antilabor laws sponsored by the authorities, and the ZZZ's liberal proposal for dealing with the economic crisis. The ZZZ leadership and its representatives in the Sejm repeatedly criticized the government's social policies and warned against the workers' rejection of the capitalist system. At the same time, the organization took a very strong stand against the danger of fascism as exemplified by the rise of the National Radical Camp (ONR). It loudly condemned antisemitic excesses throughout Poland and anti-Ukrainian discrimination. In 1937 the ZZZ added to its propaganda arsenal the slogan "people's democracy." This soon led to unjust accusations of pro-Communist sympathies and ultimately to the demise of the ZZZ.

Ajzner's work is of considerable value to students of Polish affairs. Although a Marxist scholar, his work presents a vivid picture of the activities of a dynamic syndicalist organization armed with a formidable program of social, economic, and political reform. A few exaggerations made about the importance of the Communist movement in Poland at the time do not mar seriously this valuable book, for in addition to a well-researched study of a Polish radical labor organization, the reader gets a new insight into political developments in Communist Poland.

WŁODZIMIERZ ROZENBAUM  
Indianapolis, Indiana

L. V. CHEREPNIN. *Zemskie sobory russkogo gosudarstva v XVI-XVII vv.* [*Zemskie Sobory of the Russian State in the Sixteenth and Seventeenth Centuries*]. Moscow: Nauka. 1978. Pp. 416. 3 r. 20 k.

This posthumous work of L. V. Cherepnin is a summing up of a long-term study of the Muscovite *Zemskie sobory* (nine earlier essays in different languages on this topic have been contributed by him since 1960). In this book the author traces, first of all, the historiographical and methodological background of his topic in great detail. Then in eight chronologically arranged chapters, he gives us a most exhaustive factual history of Muscovite representative assemblies from 1549 to 1683 (57 *sobory* in all). In this presentation he follows the available sources as closely as possible, submitting them to a rigorous critical analysis. While providing a factual account of these assemblies, the author also seeks to trace the

evolution of the Russian *Ständestaat* ("russkaia soslovno-predstavitel'naia monarkhiia," [p. 4]). Finally, in the concluding chapter of his book, Cherepnin undertakes a rather cursory study of more general problems pertaining to the history of *zemskie sobory*, such as their classification and periodization (a most important concern of Soviet historical science!); their relation to autocracy and to the people; the problems of "feudal ideology" (the reviewer's quotation marks!) concerning the *sobory*, as well as the international situation of Muscovy connected with them; and, finally, a very brief (four pages) comparison of the *zemskie sobory* with the "soslovno-predstavitel'nye institutions of other medieval European countries" (p. 397; "medieval" in the broad Soviet sense, the chronological range of the institutions mentioned by the author going from 1188 to 1650). Cherepnin himself, in his preface, did not go beyond considering this comparative survey as only a most modest *Problemstellung*.

While both the historiographical and the factual part of this book hardly call for any substantive criticism even on the part of a non-Marxian historian who can only recognize the great value of Cherepnin's last performance, the ideological and the comparative aspects of the book call for certain reservations. The conceptual framework, which leads to considering representative assemblies as a feature of the "feudal state" (p. 396) or even "mature feudalism" (p. 397), confuses even the most attentive and unprejudiced Western reader. It is true that so far Western historians have neglected the representative assemblies of Muscovy. No place has been found for them in *Ständische Vertretungen in Europa im 17. und 18. Jahrhundert* (1969). (In A. Marongiu's *Medieval Parliaments* [1968], quoted by Cherepnin on page 397, the Muscovite assemblies have not been mentioned either, but there were none in the Middle Ages in the Western European understanding.) So, the author's comparative correction is called for, and in this he has an illustrious predecessor: Otto Hintze, who in his "Weltgeschichtliche Bedingungen der Repräsentativ-verfassung" (*Staat und Verfassung* [1962]) accepted the classification of seventeenth-century Muscovy as a *Ständestaat*, albeit with the reservation that it had "eine viel schwächere Bildung von ständischer Repräsentation," which lacked the corporate independence of the Western estates. Such is also this reviewer's point of view, one with which, I am afraid, the late Soviet scholar might differ. All in all, however, Cherepnin's *Zemskie sobory* represents a most worthy conclusion to his remarkable scholarly career, and one must hope that it will contribute to the proper understanding of the governmental structure of Muscovy in the two centuries prior to Peter the Great.

MARC SZEFTTEL  
University of Washington



V. G. CHERNUKHA. *Vnutrenniaia politika tsarizma s serediny 50-kh do nachala 80-kh gg. XIX v.* [The Domestic Policy of Tsarism from the Mid-1850s to the Early 1880s]. Leningrad: Nauka. 1978. Pp. 246. 2 r.

This book is devoted to three topics discussed in government and public circles during the reign of Alexander II. Each topic is treated in a separate chapter that parallels the others chronologically. Chapter 1, "The Autocracy and Representative Institutions," studies the idea of introducing a representative body at the national level to participate in legislation. This section analyzes the interplay between demands made by social groups in periods of unrest and responses by bureaucrats who drafted reform proposals to calm the social unrest of the moment.

Calling bureaucratic initiatives to create representative institutions "government constitutionalism," V. G. Chernukha divides the period 1861–82 into four subperiods during which five initiatives occurred. Each subperiod is treated as a subchapter focusing on proposals submitted by P. A. Valuev (1861–63), Grand Prince Konstantin Nikolaevich (1865–66), P. A. Shuvalov (1866–75), and M. T. Loris-Melikov and N. P. Ignatiev (1878–82).

Based upon printed and archival diaries and memoirs of major government officials, correspondence among the parties concerned, and very broad reading in the contemporary and recent secondary literature (but in Russian only), this section constitutes the most systematic, detailed study of this topic available in one volume.

Chapter 2, "Unity of the State Administration," traces government and public discussion of reforming the central executive agencies during the same time period. Three subchapters are devoted to discussion of that question: (1) in 1857 during the creation of the Council of Ministers, (2) in 1861 during formulation of the Council's procedures and area of jurisdiction, and (3) in the period 1862–82.

Chapter 3, "An Income Tax and Its Socio-Political Essence," is based, in the first subchapter, upon archival materials that reveal substantial government consideration of introducing an income tax from 1862 to 1867. The second subchapter discusses zemstvo responses to proposed reform of the soul tax in 1871, and the third traces the idea of an income tax through its consideration in the Valuev Commission in 1872 and its fate to 1880.

This chapter calls attention to a topic even more important in Russian political thought than the author indicates. The public debate on the soul tax reform in the provincial zemstvo assemblies and in the press produced a general acceptance among literate, aware Russians of the idea of an income tax to replace the existing tax system. After 1871 an income tax to be paid by all classes was a standard plank in the liberals' platform.

By choosing to devote two pages to B. N. Chicherin's views on the income tax (the "liberal" whom Soviet scholars always parade out when a "liberal view" is desired), the author provides an atypical glimpse of zemstvo reaction to the government's proposed reform. In volume 22, part 1, of the Special Commission's *Trudy* (the author cites part 2 on page 224) Chernukha could have found many proposals radically different from Chicherin's. An example is the lament by the Nizhegorod County Zemstvo Board that the government's proposal "considers the word 'taxpayer' to be a synonym for the word 'peasant'" (pp. 96–97). The board favored a tax upon all classes, as did thirty of the thirty-two provincial zemstvos that considered the draft.

A couple of lapses in tonal quality do not appreciably detract from the value of this serious, soundly researched, and generally well-balanced addition to the literature on Russian political and social ideas during the reign of Alexander II.

CHARLES E. TIMBERLAKE  
University of Missouri,  
Columbia

V. A. TVARDOVSKAIA. *Ideologiia poreformennogo samoderzhaviiia (M. N. Katkov i ego izdaniia)* [The Ideology of Postreform Autocracy (M. N. Katkov and His Publications)]. Moscow: Nauka. 1978. Pp. 277. 2 r. 20 k.

Soviet historiography of conservative nationalist thought, politics, and personalities has received considerable attention since the pioneering efforts of P. A. Zaionchkovskii in the mid- and late 1960s. V. A. Tvardovskaia's work is within this tradition. Her short monograph is divided into seven parts: an introduction in which the author makes a case for an actual ideology of postreform autocracy with M. N. Katkov as its primary ideologue, and in which she opens a polemic with such Western historians as R. F. Byrnes, E. C. Thaden, and M. Katz; chapter 1, in which Tvardovskaia tries to prove that Katkov's leading publication, *Moskovskie Vedomosti*, is a governmental organ; chapter 2, in which she discusses the socioeconomic ideas of "conservative" (the quotes are hers) publicism of the 1860s through the 1880s; chapter 3, in which she deals with the "reactionary reformism" of M. N. Katkov of the 1860s and 1870s; chapter 4, in which Tvardovskaia discusses "the ideologue of autocracy in the period of the revolutionary situation in the late 1870s and early 1880s of the nineteenth century"; chapter 5, which treats counterreforms; and a short conclusion. The work is enhanced by an index of names and by footnote citations on the bottom of the pages, but it does not have a bibliography.

Tvardovskaia's case for Katkov as an ideologue of postreform autocracy is only partly convincing. Certainly there were others, such as K. P. Pobedo-



nostsev and D. A. Tolstoi, who played an equally strong role on the right wing of the government. Also the author's playing down of "conservative nationalism" and "conservative Westernism" and her failure to take into account the origins of these features in Katkov's childhood and early attraction to Schellingianism make Katkov appear only half real. She has a tendency to underestimate the impact of the Polish rebellion of 1863 and the corresponding integral Great Russian nationalism of the Moscow publisher.

Her polemics with Western historians are forced. She denies the paradox of modernization and conservative nationalism, which Western historians such as Pipes, Byrnes, Thaden, and Katz have emphasized in their works. This denial has little or no substance. She escalates her whole polemic to the level of an attack on those in the West who are not convinced by historical inevitability, especially the inevitability of the October Revolution (pp. 12-13).

On the positive side of the ledger is Tvardovskaia's use of manuscript and archival materials, which in the past Western scholars were not able to use. Also the writing of a quasi-biographical work on Katkov by a Soviet scholar is a gigantic leap forward for Soviet historiography since the beginning of the 1960s.

Tvardovskaia de-emphasizes the role of personality in history, especially in her attack on Byrnes and Katz. She also underestimates the importance of Katkov's nationalistic struggle with such dynastic cosmopolitans as Valuev and tries to show that the two men were merely two sides of the same coin (p. 72). Her arguments on this score are not very convincing.

Tvardovskaia is at her best when discussing Katkov's economic policy, but she is not far sighted enough to see Katkov's subordination of class, social, and economic interests to integral nationalism. She flatly refuses to see, for instance, that Stolypin's "wager on the strong" was a distant echo of Katkov's desires several decades before. The author further attacks Thaden and Katz for emphasizing Katkov's proclivities toward the industrialization of Russia, especially the development of railroads. She indicates that Katkov only thought of railroads as auxiliaries to agriculture, but what she does not say is that the Moscow editor was well aware of the mining and metallurgical industrial development required to support railroads.

She correctly observes that Katkov supported the interests of the Russian *pomeshchik*, but she does not mention the complexity of reasons he had for doing this. She does not indicate that this stratum was the most fervently nationalistic of all, especially after the Polish rebellion of 1863. Yet what Katkov really wanted was "*veseloslovnost'*," the unity of all estates under the crown.

Tvardovskaia also displays a genuine lack of in-

terest in Katkov's educational policies, "classicism against nihilism." In doing so she misrepresents what I said in my work on Katkov. She indicates that I said that classicism was a humanizing force, when I was only presenting Katkov's ideas on the subject.

In summation, V. A. Tvardovskaia's monograph is a welcome addition to the growing number of works, both Soviet and Western, on the conservative thought and politics of imperial Russia.

MARTIN KATZ

University of Alberta

JAMES C. MCCLELLAND. *Autocrats and Academics: Education, Culture, and Society in Tsarist Russia*. Chicago: University of Chicago Press. 1979. Pp. xiv, 150. \$14.00.

Modernization in Russia often meant borrowing from the West and in no area was this borrowing more faithful to the original than in education, particularly higher education. James C. McClelland points out that Russian education developed from the top down; the empire had an Academy of Sciences before it had grammar schools, and Russian universities became sophisticated centers of scholarship and research while the national literacy rate was the lowest among the European powers. By the end of the nineteenth century, the empire was second only to the United States in numbers of students enrolled in higher education, and the rigorous standards produced individuals capable of extraordinary achievement, especially in such fields as the natural sciences and mathematics.

The author notes, however, that the government was always uneasy with professors and students who resented interference by the state in the universities and challenged the social and political status quo. Moreover, the imperial government was never quite able to decide just what role the universities should play—whether they should be institutions for pure research on the German model or whether they should train persons of talent for the practical needs of Russian society—and this ambivalence together with the autocracy's fear of their revolutionary potential resulted in periodic tampering with curricula, faculty appointments, and student admissions. In the end, the universities emerged as centers for much of Russia's articulate disaffection, and by the early years of this century they became the launching pads for actions and demonstrations against the government.

This work raises a number of interesting questions concerning the place of education in imperial policy. One question of considerable importance is why the state did not develop a more practical system to serve the day-to-day needs of the empire. The author implies that both prestige and the belief

that scientific discoveries would contribute directly to the state's military power underlay this choice. While this undoubtedly had validity, it appears to discount the values of the scholarly community itself, which, if A. V. Nikitenko is to be considered typical, accepted pure scholarship and research as the natural and exclusive role for the university. Professors trained either in Germany or in the German academic tradition could hardly be expected to seek a university structure or purpose substantially different from that of their model, and this undoubtedly served also to narrow their views on what constituted appropriate preparation for university study, thereby critically affecting secondary curricula. One is tempted to wonder what might have been the result if the government had permitted the universities to govern themselves as Russian professors wished. In such a circumstance would the academic minds have been any more anxious than was the state to abandon their privileged role and substitute for it a mission of teaching peasants and artisans to become mechanics and accountants?

This work often tends to ramble, and the author makes allusions without regard to the needs of the more general reader. The system of primary education is treated in only cursory fashion, and McClelland pays insufficient attention to the vast array of schools not within the purview of the Ministry of Education, particularly those in the army, where practical curricula did evolve and where some of the most significant assaults upon illiteracy were made. Nevertheless, we must be grateful to the author for providing a framework for more specialized studies in the future and for provoking some very interesting questions that lie at the very heart of the history of the empire.

FORREST A. MILLER  
Vanderbilt University

N. M. DRUZHININ. *Russkaia derevnia na perelome, 1861–1880 gg.* [The Russian Countryside at the Turning Point, 1861–80]. Moscow: Nauka. 1978. Pp. 286. 2 r. 50 k.

The succinct title of N. M. Druzhinin's latest monograph conveys a topic, a time, and a thesis. Note, however, that the "Russian countryside" it advertises is limited to the territory of the "main directing nucleus of the Russian state at the time" (p. 4)—thirty Great Russian provinces of central European Russia. The period covers the pivotal first decades of emergence from serfdom, decades spanning the interval between the "revolutionary situations" of 1859–61 and 1879–81.

The work essentially synthesizes, and to some extent supplements, previous scholarship on the con-

dition of the peasantry in the era of great reforms. The objective is to demonstrate concrete linkages between the emancipation, economic developments, and the emergence of a new political crisis. This is a complex task, but the author bears impressive credentials. Best known for his classic study of the 1837–41 reform of the state peasant administration, the indefatigable Druzhinin (b. 1886) has been a productive scholar of varied interests.

The first four chapters of his book detail economic and institutional aspects of the emancipation and describe the relative status of different categories of peasants. The remaining four deal with peasant landholding, the peasant economy, regional differences, and the crisis of 1879–81. A brief conclusion concisely summarizes the entire presentation. The sources cited are primarily archival and published documentary materials, but recent research of Soviet scholars is broadly utilized.

Druzhinin's contention that the peasantry fell under mounting stresses after emancipation rests on basically familiar ground. He points to initial land losses, excessive redemption payments, increasing inadequacy of allotments, disparity between land income and taxation, and social bias in state policies. Most of the argument has been well rehearsed in the literature, but Druzhinin moves it forward by emphasizing the early impact of these factors, assembling some new evidence, and offering some provocative reappraisals (for example, of peace arbitrators). Sixty tables of data provide information ranging from the number of peasants at the time of emancipation to the reduction of redemption payments in 1881. Despite some lapses (inadequate discussion of communal landholding; a top-sided view of peasant self-government), the work projects a persuasive picture of escalating economic pressures created and compounded by government policies.

Yet the picture has its brighter side. The emancipation, as Druzhinin repeatedly observes, secured the personal liberation of the peasants and simultaneously released great potential for economic development. Some significant gains, in fact, are shown to have been achieved in these decades.

The historical prospect, then, is one of countervailing tendencies. Economic growth was providing new and expanding opportunities for some peasants, but the burdens on the peasantry as a whole were increasing. Agricultural productivity was rising, but production per capita was declining. As Druzhinin presents it, the contradictions were the result of tensions between surviving elements of the old feudal system and an emergent capitalist formation.

If peasant economic difficulties are convincingly tied to the emancipation, they nonetheless fail to provide an adequate explanation of the 1879–81 crisis that Druzhinin himself describes as "sociopoli-

tical." The chapter dealing with this topic is the most problematic in the book. By all accounts (including this one) the peasants were not politically active; it was the terrorism of the populist intelligentsia that created the immediate crisis. And, as Druzhinin concedes (pp. 273–74), the radicals were a small group with weak ties to the peasantry. A severe methodological (or logical) weakness of the work at this point is its failure to demonstrate either that radical politics were directly related to peasant economics or that the government perceived them as such. The case is more proclaimed than proved.

Whether from fear of revolution or simply because peasant problems were becoming more evident, the government did take measures (both before and after the 1881 assassination of Alexander II) to alleviate the situation of the peasantry. With what results? Unfortunately, Druzhinin's story ends right here. Concluding pages set the stage for a tale of ongoing and deepening agrarian crisis, leading to the ultimate revolutionary situations of 1905 and 1917. But the book itself would suggest that in the early 1880s the future of the Russian countryside had by no means been foreclosed.

DOROTHY ATKINSON  
Stanford University

P. G. RYNDZIUNSKII *Utverzhdenie kapitalizma v Rossii, 1850–1880 gg.* [The Consolidation of Capitalism in Russia, 1850–80]. Moscow: Nauka. 1978. Pp. 293. 2 r. 30 k.

P. G. Ryndziunskii has been an active, and frequently very perceptive, historian of the late-nineteenth-century Russian economy and society. The purpose of his latest monograph is to draw together the findings of specialized enquiries into industry, trade, finance, agriculture, and so forth, and to relate them to the formation of both bourgeoisie and proletariat during the early stages in the development of capitalism. The study is restricted to conditions in European Russia. The book's contribution lies not so much in bringing to light new evidence on the "consolidation of capitalism in Russia" in the 1850–80 period—though some archival data are presented and considerable use is made of a variety of nineteenth-century statistics—but in synthesizing a portion of the huge volume of Soviet scholarship on the topic.

The book is divided into three parts: the demise of serfdom; the formation of capitalist structures; and the formation of the classes of capitalist society. Chapters in the first part deal with the characteristics of trade and commerce, including the formation of a European Russian market, industrial development and the agrarian economy—all during

the decade or so prior to the emancipation of 1861. The final chapter in the section summarizes, yet again, the illiberal consequences of the emancipation and sets the stage for viewing, among other things, the reductions in cultivated area and the imposition of redemption payments as factors in fueling rural-urban migration and swelling the ranks of the lumpenproletariat. Emphasis throughout is placed on an orthodox interpretation of Marx. Thus, historians like G. P. Asaev, who have ventured to interpret the beginnings of industrial capitalism in terms of simple technological change rather than in terms of control over the means of production, are duly upbraided (pp. 25–26).

The second part of the book examines these same themes up to the 1880s. By this time more specialized market areas had emerged, much facilitated by the rapidly expanding railroad network, and with market specialization and industrialization came a complex system of regional interdependencies. The rapid development of rent relationships is accorded special attention as one measure of the intensification of capitalism—and by the 1880s there is no question that they were a prominent feature of the rural economy. Ryndziunskii regards the industrial revolution in Russia as having been completed by the mid-1880s. In the ensuing decades industrial capitalism intensified, to be sure, but the period 1850–80 was the critical one in terms of its taking root in Russia. While technical innovation in industrial production gained momentum, some spheres of production, such as textiles, still made extensive use of homeworkers (*nadomniki*) in the 1880s.

The final part of the study discusses the formation of bourgeoisie and proletariat. The growing concentration of industrial and merchant capitalism is highlighted. A simple correlation analysis clearly shows how closely the two are linked spatially throughout European Russia. Among the institutions administering credit, "such great spatial concentration . . . is evidence of the high degree of development of the capitalist system in the country" (p. 240). In describing the formation of the proletariat, Ryndziunskii focuses his attention on the conditions in the countryside forcing peasants to seek employment outside agriculture, either seasonally or permanently. It is clear that the link with the village is maintained. The question therefore arises: just what were the consequences of this continuing link with the village among so many urban factory workers in terms of their becoming fully assimilated into the proletariat? But Ryndziunskii does not stray from the orthodox view that a factory worker *ipso facto* is proletarian. This may make it easier to measure the development of the proletariat, but it does not do much to recreate an authentic picture of time and place. In summary, the book serves as a useful, but largely predictable, survey of

economic and social change during this important period in Russian history.

JAMES H. BATER  
*University of Waterloo*

ROBERT N. NORTH. *Transport in Western Siberia: Tsarist and Soviet Development*. Vancouver: University of British Columbia Press and Centre for Transportation Studies. 1979. Pp. 364. \$22.00.

Robert N. North has produced a substantial monograph that embraces not only various modes of transport, especially railways, but also other aspects of economic development, including industry, agriculture, and trade, and their mutual relationship from the 1850s up to 1975. About one-third of the text treats the prerevolutionary era, with almost equal space being given to the periods before and after the coming of the Trans-Siberian Railway in the 1890s. The remainder is divided into chapters of about thirty pages each, dealing with successive stages of Soviet rule: to the end of the NEP period, the first three Five-Year Plans, the Second World War and the last years of Stalin's regime, the Khrushchev era, and finally the Brezhnev era. The last part of the book contains appendixes that present *inter alia* numerous meticulously researched and compiled tables and maps. There is also an abundance of maps throughout the text.

The main part of each of these chapters is devoted to a very detailed description of economic developments taking place during the period under discussion. The reader is treated to a wealth of facts and statistical data that at times becomes almost overwhelmingly informative. At the end of each chapter there is a brief summary and a discussion of continuity or change with respect to the preceding period. There are also attempts to compare and contrast the economic development of the region with that of other parts of the USSR, especially the Urals and Eastern Siberia but also some of the industrial areas of European Russia.

Because of the time span involved, the author's treatment of the pre-1914 era has to be somewhat superficial. He seems to be most at home in discussing post-1945 events. His sections on developmental problems in chapter 8 and on continentality and economic development in chapter 9 are the most analytical and broadly interpretive and hence will hold the greatest interest for the general reader. The discussion of recent developments in the Siberian oil industry will also be of considerable interest in view of recent world events. However, all parts of the book contain a wealth of factual and bibliographical information that will be welcomed by specialists wishing to delve more deeply into specific topics.

The author has an extensive command of his bibliography, both Russian and English (although he does not utilize several relevant secondary works on Russian railways in the nineteenth century). In addition to monographs and journal articles, the author draws upon tsarist and Soviet government publications. But complete, reliable, and consistent data for all periods and branches of industry are difficult to obtain. Therefore, on some specific points the author must resort to suppositions, nearly all of which seem quite reasonable.

In summary, North's book, although narrow in geographical focus, is well researched and informative. Many more such specialized works should be written if our understanding of Russian and Soviet economic development is to be increased.

RICHARD MOWBRAY HAYWOOD  
*Purdue University*

MANFRED HILDERMEIER. *Die Sozialrevolutionäre Partei Russlands: Agrarsozialismus und Modernisierung im Zarenreich (1900–1914)*. Cologne: Böhlau Verlag. 1978. Pp. xviii, 458. DM 98.

The party of bombthrowers and agrarian protest has at last found a historian in Manfred Hildermeier. His valuable study provides a useful introduction to the works of Oliver H. Radkey and others who investigated the vicissitudes of the Party of Socialist Revolutionaries (PSR) during the Russian Revolution.

The monograph is based on a wide range of printed sources in Russian and other languages. In addition, Hildermeier has made good use of the PSR records in the International Institute for Social History in Amsterdam and of the Okhrana files at the Hoover Institution at Stanford University. On the other hand, he does not seem to have consulted the material on the PSR in the Archives Nationales and the Archives de la Préfecture de Police in Paris.

The book begins with an account of the attempts to form the party in the 1890s. This is followed by a detailed analysis of the party's program and stand on many issues, although not on the nationality, women's, and cultural questions. Three well-documented chapters are devoted to the spread of the PSR network on the eve of the 1905 Revolution, the role of the party in 1905–07, and the strengths and weaknesses of regional party organizations. A short account of party finances precedes an analysis of the background of PSR leaders and activists. Unfortunately, neither here nor elsewhere in the book does the author examine in depth the contribution of students to the development and image of the PSR. The remaining chapters deal with the PSR in the years of the Stolypin reaction and on the eve of the First World War. Hildermeier rightly pays much



attention to differences of opinion among prominent SRs and to the effects of terrorist activities in which the party engaged and which Hildermeier considers counterproductive.

In a regrettably brief conclusion Hildermeier ponders over the reasons for the failure of the PSR, which, he thinks, was more prone to factionalism than other major political parties in Russia. The failure to grasp the importance of organization also hurt the PSR, although the author admits that the times were not ripe for an "agrarian mass party" (p. 400). The PSR program receives no accolade from Hildermeier. Its utopian character harmed the cause of Socialist Revolutionaries. A populist party could not act as a modernizing force in Russian society once capitalism had evolved beyond a certain point.

IVAN AVAKUMOVIC  
*University of British Columbia*

L. P. LIPINSKII. *Stolypinskaia agrarnaia reforma v Belorussii* [The Stolypin Land Reform in Belorussia]. Minsk: Belorusskii Gosudarstvennyi Universitet. 1978. Pp. 221. 2 r. 10 k.

In this study of the Stolypin reforms in Belorussia, L. P. Lipinskii claims to have investigated for the first time both their premises and character. He does not, however, depart too far from the accepted Soviet interpretation that the Stolypin reforms there, as everywhere in Russia, were the autocracy's answer to the Russian Revolution of 1905-07—an attempt to neutralize the influence of the revolution and to destroy the basis for revolutionary upheavals in the future. In this view the character of the reforms was clearly reactionary. This accords well with the author's view that, given the demonstrable development of capitalism in the rural economy of the area, the social stratification in the countryside, and the tendency toward settlement on private farmsteads well before the revolutionary years of the early twentieth century, they were little more than attempts to shore up the landlord regime.

It is little wonder, then, that Lipinskii, drawing largely on secondary materials, further argues that the results of the Stolypin reforms in Belorussia were the mobilization of the landowners, the concentration of further economic power in their hands, and the subsequent proletarianization of the peasantry with attendant class polarization. He attempts to buttress these arguments with a number of tables that, while giving some quantitative atmosphere to his arguments, are not always rigorously analytical and frequently raise as many questions as they are intended to resolve.

Perhaps the most interesting aspect of Lipinskii's work, however, is his effort to critique the "unscien-

tific views of bourgeois historians" who, he claims, have in their respective works on this era "idealized Stolypinism." In fact, the larger question at issue is one of immense concern for those interested in the historiography of this period since its juxtaposes Lipinskii's traditional Marxist-Leninist interpretation with the Western (and especially Anglo-American) theory of "modernization." He singles out for attack the works of Theodore von Laue, George Yaney, Cyril Black, Alexander Gerschenkron, Leopold Haimson, and George Takmakov. Collectively and individually these historians and their lesser-known American and British colleagues are accused of a multitude of shortcomings and outright concealments that range from underestimating the revolutionary potential of the peasantry to ignoring the survivals of serfdom in the countryside and the acuity of the struggle between the antagonistic forces at work in Russian society. Such views he brands as "absurd" and "falsifications of historical truth."

Given such rigid viewpoints and categorizations, the book may be interesting at one level, but as a useful and objective study of the Stolypin reforms in Belorussia, it leaves much to be desired.

GEORGE E. SNOW  
*Shippensburg State College*

NEIL HARDING. *Lenin's Political Thought. Volume 1, Theory and Practice in the Democratic Revolution*. New York: St. Martin's Press. 1978. Pp. 360. \$25.00.

The dustjacket of Neil Harding's study depicts a pensive Lenin, his narrow eyes focused somewhere in the distance, perhaps in the future. The appropriateness of this portrait in red becomes quickly apparent as one moves through the text. Lenin's political thought was never a disembodied philosophy of the state or political morality; it was neither a fixed extrapolation and application of Marxist prescriptions nor an ultimately flexible hodgepodge of ex post facto rationalizations. Rather, argues Harding, it was a remarkably constant and consistent elaboration of political strategies based on a fundamentally orthodox Marxist analysis of the socioeconomic situation in tsarist Russia; it was the evolution of theory and practice appropriate for the ever-anticipated democratic revolution.

Just as Lenin in most of his major writings set up targets for critique and laid out positions to be defended, so Harding begins by attacking the "conventional wisdom which runs through almost all Western commentary, criticism and biography of Lenin" (p. 1). That wisdom holds that Lenin was an inconsistent, unorthodox theorist of Marxism and, as a revolutionary thinker, closer to Russian Jacobins like Tkachev and several Populists than to Marx, Engels, and Plekhanov. This widely held



view has neglected to examine adequately Lenin's economic and social analyses that, in Harding's reading, "provide the clue to coherence or consistency in his more expressly political strategies" (p. 4).

Harding contends that in the period 1894–1914 Lenin held a view of Russia's revolutionary potential based on his study of the level of capitalist development in Russia. Just as Russian economic evolution moved through phases, so did the social development of its constituent classes. For this reason political strategies and organizations also had to evolve. Up to World War I Lenin's political outlook was based on his assessment of "Russia's ripeness for a radical democratic revolution" (p. 6). After 1914 he began to doubt capitalism's progressive potential and concluded that imperialism had created conditions for an international *socialist* revolution. In his close analysis of Lenin's prewar writings, Harding provides several useful correctives to the usual misreading of *Chto delat'*? ("What is to be Done?"). He argues that this work was limited in its applicability to the specific political situation at the time it was written, that Bolshevism, organizationally and strategically, changed later in response to the mass upheavals of 1905–1907, and that this sacred text of the "party of a new type" gives one no insights into explaining the phenomenal success of the mass appeal of the Bolsheviks among workers in 1912–1914. The consistency of Bolshevism stemmed from Lenin's constant adherence to Plekhanov's original notion that in Russia the proletariat would play the leading role in the democratic revolution in place of the pusillanimous local bourgeoisie.

Although Harding stays perhaps too close to Lenin's own words and does not investigate thoroughly enough the attitudes of his opponents and the actual historical context in which Bolsheviks and Mensheviks operated, he has given us one of the most original reconsiderations of Lenin's thought in recent decades. One might question why he does not use Leopold Haimson's earlier analysis or ask, more fundamentally, whether Lenin's views accurately reflected reality. But given the more modest scope of his purpose, Harding has managed to do for Western scholarly literature and Lenin what Soviet historians under Khrushchev did for many of Stalin's victims. He has rehabilitated Lenin as a serious and consistent Marxist thinker, as a political strategist who based his actions not on mere expedience but on a thoroughgoing consideration of the social and economic limitations of his own time.

RONALD GRIGOR SUNY  
Oberlin College

ROBERT SERVICE. *The Bolshevik Party in Revolution: A Study in Organisational Change, 1917–1923*. New York: Barnes and Noble. 1979. Pp. 246. \$24.50.

Robert Service has written a concise but factually rich history of the Bolshevik party from the revolution to 1923, the time of the major defeat of Trotsky by the troika of Zinoviev, Kamenev, and Stalin. Its specific virtue is that it does not confine itself to contests of personality and policy but uses them as a backdrop to describe the evolving structure of the party. The account includes the thoughts and polemics of the participants as they sought to comprehend and direct this changing structure. Service presents the change as an "internal metamorphosis" from "democratic anarchism" to monolithic centralism (pp. 2, 4).

The author's standpoint is that of democratic socialism; hence the evolution he describes is one of tragic degeneration. What are its causes? His implicit thesis is that no single factor or category of them constitutes an explanation: neither Lenin's will and his organizational theory, nor so-called objective circumstances, such as economic conditions and the Russian political tradition, nor the imperatives of large-scale organization, nor morale suffice. By means of historical reconstruction, Service seeks to juxtapose the various factors, to reveal the tensions between and among them and the specific contribution of each to the outcome. He brings to bear a critical understanding of social theory, especially the ideas of Weber and Michels. The larger problem he addresses and for which the narrative constitutes evidence is the roots of Stalinism. In what sense and to what degree is Stalinism a consequence of Leninism?

The conception of the narrative is superior to its execution. Some of the weakness probably stems from the publisher's effort to reduce costs—hence the absence of a discussion of sources and a historiographical discussion that is too elliptical and sometimes fails to provide even the titles of works discussed. Other shortcomings are the author's own. In the early chapters he relies too greatly on secondary sources. His grasp of the secondary literature on the underground period is incomplete. A conventional St. Petersburg-centered account results, which does not introduce the reader to the subject. His portrayal of 1917 is equally narrow in focus and conventional. The pace quickens and the author assumes firm command as he takes us into the party crisis of 1918.

The descriptions are sometimes overdrawn and the contrasts too stark. The beginning point is Bolshevism as "democratic anarchism"; the end point is Bolshevism in 1923 in a state of near-perfect centralization. The author has exaggerated certain tendencies in both cases. In the first place, even if Bolshevism was swayed by tides of anarchy in 1917, it did not succumb entirely. If it had, how could it have mobilized the mass institutions created by revolution—factory committees, soviets, and so forth—a process the author correctly calls to our attention?

In the second place, the dominant image of the party in 1923 is one of unremitting centralization; "regimented grotesquerie" (p. 210) is the author's phrase. Hindsight, however, informs us that the party had much further to travel down this road. In effect, Service has provided two models of organization in place of descriptions of concrete realities.

Perhaps these shortcomings are overstated. Service's work is successful both as a narrative and analysis: it will clarify understanding of the early development of Bolshevism, of political and organizational behavior in general, and of the nature of historical causation.

GEORGE ENTEEN  
Pennsylvania State University

E. G. GIMPEL'SON. *Velikii Oktiabr' i stanovlenie sovetskoi sistemy upravleniia narodnym khoziaistvom (noiabr' 1917-1920 gg.)* [Great October and the Formation of the Soviet System of Managing the National Economy (November 1917-20)]. Moscow: Nauka. 1977. Pp. 310. 2 r. 30 k.

This book assesses the establishment and development of the Soviet system of economic management from the revolution in November 1917 until the end of the period of War Communism (early 1921). Unlike many Soviet authors writing on this period, E. G. Gimpel'son presents an overall view of the development of the system of economic management and stresses how organizations changed over time and how they interacted with each other. His book is thus crammed with compact detail, most of it taken from works published in the 1920s and from later studies. Still, the author's outlook prevents the work from being merely derivative, although at times it reads like a text on administrative law.

Overall, the work is a defense of Soviet policies, with the expected fulsome praise of Lenin (described, for instance, as "a brilliant scholar and great organizer"). The author manages a certain frankness in discussing some of the problems encountered in implementing economic management, without actually admitting that, for the most part, utter administrative confusion in fact existed at the time. He fully supports the now standard Soviet view that the Civil War, foreign military intervention, economic blockade, and other factors forced the regime to adopt the policy of War Communism and what he calls "several unusual organizational forms and methods of leadership" as temporary expedients, thereby implicitly denying any role of ideology in the undertaking of these measures.

Gimpel'son is concerned more with the organization of economic management than with economic consequences. He thus focuses attention not so much on the nationalization of almost all industry

and the seizure of peasant "surpluses" as on the difficulties in coordinating the work of the *sovmarkhozy* (regional economic councils, organized on the territorial-production principle) with that of the eventually predominant *glavki* (central directorates, organized on the branch-production principle). He treats sympathetically the inability of VSNKh (Supreme Council of the National Economy) to cope with all the tasks of coordinating and directing the economy during this period and emphasizes the "deciding role" of the Council of Workers' and Peasants' Defense (later called the Council of Labor and Defense) under Lenin in accomplishing this.

Chapters are devoted to the organization of management in industry, transport, and construction, the leadership of agriculture, organizational forms and methods of mobilizing labor and material resources, first steps in planning, and the leading role of the working class in economic management. In maintaining the conventional Soviet view on the role of the working class, the syndicalist tendencies of the unions at the time are virtually overlooked, and opposition to Lenin on the issues of one-man management and the subordinate role of the unions is minimized. However, of interest are Gimpel'son's data indicating that workers were more numerous than bourgeois specialists in industrial management at the end of 1920, if not before, and his view that workers played a greater role in economic management than has been recognized.

The value of the book is that, within the limits of the accepted Soviet viewpoint, it provides a comprehensive overview of attempts to implement economic management during this chaotic period. Moreover, the material on interrelations between organizations is often interesting and informative. It is thus a useful foundation study of the period, although it leaves a number of important questions (from the Western viewpoint) unasked and unanswered.

EVERETT M. JACOBS  
University of Sheffield

TEDDY J. ULDRICKS. *Diplomacy and Ideology: The Origins of Soviet Foreign Relations, 1917-1930*. (Sage Studies in Twentieth Century History, number 9.) Beverly Hills, Calif.: Sage Publications. 1979. Pp. 239. Cloth \$22.50, paper \$9.95.

Teddy J. Uldricks has set himself the task of providing a chronicle and analysis of the origins and early development of the Soviet diplomatic corps. In this he has succeeded admirably, for he has produced a clearly written and well-developed study of the structural and organizational development of the People's Commissariat of Foreign Affairs (Narkomindel) during the tenure of its first two commissars, Lev Trotskii and Georgii Chicherin.

Uldricks begins with a description of the chaotic conditions that surrounded the birth and early history of the Narkomindel, when former Provisional Government diplomats refused to turn over Russian embassies and consulates abroad and when the new Soviet state maintained no diplomatic relations with foreign governments. He then discusses the revival of diplomatic activity during the period of the New Economic Program and the significant expansion and reorganization of the commissariat during these years along lines parallel to those followed by other countries.

One of the more interesting portions of *Diplomacy and Ideology* treats the personnel of the diplomatic corps recruited under Trotskii and Chicherin. Given the refusal of the vast majority of pre-Soviet Russian diplomats to serve the new state, the Narkomindel became the most completely "Bolshevized" department of government. The personnel of the commissariat, however, was much more highly educated and much more heterogeneous than the members of either the Communist Party or the government as a whole (87 percent of personnel who served from 1917 to 1930 had at least some university training and only 42 percent were Great Russians).

In his treatment of the relationship of the commissariat to political developments in Soviet Russia, Uldricks argues that the Narkomindel enjoyed a relatively great degree of freedom in conducting foreign policy during the power struggles of the mid-1920s, so long as it operated within the general guidelines established by the Politburo. It was more than a mere executor of policy established by the party and was able to influence the overall development of Soviet foreign policy.

Uldricks disagrees with a number of what have become "accepted" interpretations of the early development of Soviet diplomacy. For example, the Narkomindel never became an active center of oppositional strength, nor did it become a primary "dumping ground" for defeated political figures, even though a small number of diplomatic appointments did apparently result from political disgrace.

The author concludes with a discussion of the destruction of the diplomatic corps during the Great Terror of the 1930s. He estimates that, of the top levels of commissariat officials, more than 62 percent—and possibly as high as 70 percent—died during the Terror. Since 14 percent had either died or defected before 1935, the impact of the Terror on the commissariat was even greater than a figure of 70 percent would indicate.

*Diplomacy and Ideology* makes an important contribution both to the early history of Soviet foreign relations and to our better understanding of the evolution of the governmental apparatus of the Soviet state. The author has used effectively a vast array of

both primary, including archival, and secondary sources to document and explain the evolution of an important instrument in Soviet policy.

ROGER E. KANET  
University of Illinois,  
Urbana-Champaign

EDGARS DUNSDORFS. *Pirmās latviešu bībeles vēsture* [The History of the First Latvian Bible]. Summary in German. Lincoln, Nebr.: The Latvian Evangelical Lutheran Church in America. 1979. Pp. 233. \$28.00.

Edgars Dunsdorfs is one of the most distinguished Latvian economists and historians and certainly the most prolific one. He started his career at the University of Latvia and continued his activities at the Baltic University in Germany and the University of Melbourne in Australia. His books and articles in scholarly journals are too numerous to mention here. Although his basic field is economic history, he has done most of his research in general history, starting out in the seventeenth century but later extending his interests in both directions to earlier and later centuries, dealing even with post-World War II problems.

In this volume—*Pirmās latviešu bībeles vēsture*—Dunsdorfs has returned again to the seventeenth century. We might wonder what kind of history one can write about translation of the Bible into one or another language. Dunsdorfs has proved that one can not only fill 233 pages with a text but also make it interesting and valuable reading. In addition to this, Dunsdorfs's present volume can also serve as an exceptionally good example of fine historical scholarship of the highest caliber. Basically the book deals with the first translation of the Bible into the Latvian language, officially published in 1689 in Riga under the auspices of, and dedicated to, Charles XI, the king of Sweden. In preparation for this study the author has utilized completely unknown archival sources found in Stockholm and Riga, and he has critically evaluated all existing literature on the subject. He has also dealt in considerable detail with the principal persons involved in the sponsorship and publication of the Bible, revealing their personalities and various activities. Particular attention quite naturally has been paid to the translator himself, Probst Ernst Johann Glück (1654–1706), whose stepdaughter Martha, by the way, later became the mistress and finally the wife of Peter the Great of Russia and subsequently the Empress of Russia under the name of Catherine I. Besides presenting to the reader entirely new information on the planning of the translation and the publication of the Bible in two Estonian dialects as well, the book also contains a wealth of informa-

tion on the political, social, cultural, and economic conditions in the Swedish Baltic provinces of Estonia and Northern Latvia in the seventeenth century. Footnoting is extremely meticulous and detailed, compensating for the lack of a separate bibliography. Sixty-eight pages of the text are devoted to reprinting of original documents or presenting their facsimiles. The book also contains detailed indexes of persons, place names, and subject matters, as well as a good summary in German that, along with the texts of documents in German and Swedish, might be useful to Western scholars.

EDGAR ANDERSON  
San José State University

### NEAR EAST

JOHN B. WOLF. *The Barbary Coast: Algiers Under the Turks, 1500 to 1830*. New York: W. W. Norton. 1979. Pp. xii, 364. \$16.95.

The emergence in the 1980s of a revived militant Islam as a powerful force in world affairs, buttressed by vast petroleum reserves and a key strategic location between established power blocs, underscores the need for understanding of the historical antecedents of Islamic-Christian interaction not only for policymakers and academicians but also for the general public as well. John B. Wolf's study of the corsair state of Algiers meets this criterion in general and as such is a welcome addition to the historical literature available in English on North Africa. Although he has overlooked at least one "general book in English" (by this reviewer), his book provides a thorough description based on European archival and national libraries and the extensive accounts of Europeans who dealt with the corsair state during its three centuries of vigorous existence. The book's value is enhanced by a fine selection of photographs, although a location map of Algiers in North Africa would have helped.

Wolf begins his study with a brief analysis of the fifteenth-century Maghrib and Mediterranean political conditions that permitted successive Spanish and Ottoman conquests of Algiers. The balance of the book examines the formation of the "regency," its government, social structure, and corsair and prisoner communities, and Algerian-European relations through 1830 and the final French conquest.

Unfortunately, the accuracy of Wolf's "portrait" of Ottoman Algiers is badly flawed, the result of total reliance on European sources and the absence of in-depth analysis of internal Maghribi relationships. The Ottoman documentation clearly indicates that the Algiers case was anything but a "300-year government by a foreign army of occupation" (p. xi). The author's unfamiliarity with Islamic North African society may explain in part the appalling num-

ber of geographic, religious, political, and other factual errors and inconsistencies. The references to Muslims as "Mohammedans" (p. 2 et seq.) alone would entitle the author to 100 lashes in the *Bedestan* (prisoner-market) of Algiers. The Kabylia region is consistently referred to as a tribe and its sedentary cultivators as nomads. The author also uncovers three "sects" of Islam previously unknown to Islamic historicity (p. 95). What is really regrettable is that after five centuries of interaction the same old myths and misconceptions about the Islamic world are still purveyed from the same tired platform.

WILLIAM SPENCER  
Florida State University

WERNER ZÜRRER. *Persien zwischen England und Russland, 1918-1925: Grossmachteinflüsse und nationaler Wiederaufstieg am Beispiel des Iran*. Bern: Peter Lang. 1978. Pp. 501. 98 FR.

The recent Iranian revolution, the demise of the Pahlavi monarchy, and the continuation of the "Great Game" in Central Asia between the Soviet Union and the United States (in place of Britain) have some of their roots in the rivalry between Britain and Bolshevik Russia over Persia during the immediate post-World War I era. The subject has been dealt with by the former Polish diplomat George Lenczowski in his survey of *Russia and the West in Iran, 1918-1948* (1949) and (inadequately) by N. S. Fatemi in his semi-official study of Anglo-Russian power politics in Iran between 1917 and 1923, but these studies were not solidly based on British, German, French, and American archival materials and some important Soviet publications. Werner Zürrer, who has published excellent studies of great power rivalry in the Caucasus and the Transcaspian region during the 1918-21 period, has attempted a detailed account of Anglo-Russian rivalry and the rise of the Pahlavi dynasty in Persia from 1918 to 1925.

During World War I, Russian and British forces repeatedly invaded and occupied parts of Persia to terminate German influence in the country. As the Russians withdrew after the Bolshevik coup in 1917, Persia lapsed into virtual anarchy until a British force occupied the northeastern region in early 1918 to block an alleged Bolshevik threat to India via Persia. Following the end of the war, the Persian government dispatched a delegation to the Paris Peace Conference that asserted claims to the Transcaspia, parts of Russian Central Asia, and a frontier on the Euphrates River. In early August 1919 the British concluded an agreement with Tehran that reaffirmed the independence of Persia and provided technical and financial assistance. Although the ac-



cord was rejected by the Majlis, Persian mercenaries assisted British and White Army forces in their war against the Bolshevik regime.

As the Red Army triumphed in the Transcaspiya and British forces withdrew from northern Persia in January 1921, an officer of the Persian Cossack brigade, Reza Khan, seized Tehran and concluded peace with the Bolshevik regime. To counter British influence in Persia, the Soviet government granted a generous settlement that, with the assistance of an American financial mission and the support of the mullahs, enabled Reza Khan to assert his authority over the country. He was especially successful (with Russian support) in quelling British-inspired separatism in the oil-rich southwestern part of Persia. By the end of 1925, Reza Khan had established himself as dictator, deposed the last shah of the Kajar dynasty, and proclaimed himself the Shahanshah Reza Pahlavi. It was the beginning of a dynasty that endured until the Khomeini revolution.

Zürner's study is not only thorough and well balanced but also highly interesting. It supplements F. Kazemzadeh's *Russia and Britain in Persia, 1864-1914* (1968) and will remain the definitive work on the subject until data from the Soviet archives provide an additional dimension and some new interpretation for the story.

J. O. BAYLEN  
Georgia State University

## AFRICA

J. D. FAGE, editor. *The Cambridge History of Africa*. Volume 2, *From c. 500 BC to AD 1050*. New York: Cambridge University Press. 1978. Pp. xvii, 840. \$69.50.

This is the fourth volume, out of a projected eight, to appear of the *Cambridge History of Africa*. When the set is completed, African history will have come of age. Not taught on a regular basis in universities, even in Africa, until the 1960s, African history is now blessed with the sanction of academic approval that is provided by the "Cambridge History" imprimatur. This set is very much in the vein of previous Cambridge histories, a sound but very orthodox and somewhat unimaginative work of reference. Volume 2, edited by J. D. Fage, covers the period from around 500 B.C. until A.D. 1050. Its eleven lengthy chapters synthesize what is known of the history of the continent on a regional and chronological basis. Some of the most valuable aspects of the volume are the comprehensive bibliographic essays and bibliography, which, together with the superb index of 70 pages by Marion Johnson, make up nearly a fifth of the total length.

Much of the source material for the present volume is nondocumentary. In a rather curious way

the editor, J. D. Fage, classes as a "beginning to history" the use of iron to make weapons and tools (p. 2). Although certainly a useful periodization marker, a technological change of this nature does not mean history. African historians have a tendency to begin their African history courses at the beginning of the Iron Age, but this is still a period when most of Africa's past is chiefly accessible by the tools of the archeologist, the word lists of the linguists, the fossil plants of the paleobotanists, and the interpretations of the ethnographers.

Fage further calls this period "definable methodologically as a work of what has come to be known as proto-history." When this term was originally used by European prehistorians at the turn of the century, it was reserved for such periods as the Belgic occupation of parts of southern Britain for which there were accounts by historians like Julius Caesar or data derived from numismatics but no internal written sources. Except for North Africa, parts of Ethiopia, and very small areas of West Africa and the East African coast, there are not even remotely indirect documentary sources for at least two-thirds of the continent. At an international conference concerned with terminology at Burg Wartenstein in 1965, prehistorians working in Africa decided to drop the term, and it is a pity that the historians have not followed suit.

In the manner of most Cambridge histories, the book consists of a series of largely independent essays starting off with the "Legacy of Prehistory" by J. Desmond Clark, which, although thorough, occasionally lapses into too detailed trait description. This essay is followed by chapters on North Africa by R. C. C. Law, the Nilotic Sudan and Ethiopia and Christian Nubia by P. L. Shinnie, trans-Saharan contacts and the Iron Age in West Africa by Raymond Mauny, the emergence of Bantu Africa by Roland Oliver and Brian Fagan, the Christian period in the Mediterranean by W. H. C. Frend, the Arab conquest and the rise of Islam in North Africa and the Fatimid revolution by Michael Brett, and finally the Sahara and the Sudan from the Arab conquest of the Maghrib to the rise of the Almoravids by Nehemia Levtzion. There are few attempts to outline the major themes on a broad basis except in the chapter on Bantu Africa, no comparative study of the demographic aspects of the continent as a whole, and a paucity of discussion of recent theoretical ideas concerning the emergence of complex societies, the development of exchange systems, or the underdevelopment theory.

The authors do not draw together the artistic achievements of sub-Saharan Africa; one looks in vain for photographs or even adequate coverage of the terracotta art of Nok in Nigeria or of the Lydenburg heads of the Transvaal, which are surely as significant in terms of intellectual or religious his-



tory as pots and stone tools are to economic and technological history. One also looks in vain for a challenge to, or an analysis of, the provocative views of Cheikh Anta Diop on the relationships between Black Africa and the Nile Valley civilizations. One hopes that some discussion of his theories will be included in volume 1, as they have influenced many African scholars, particularly in francophone Africa, as well as black historians in America.

The most interesting chapters are those dealing with North Africa. There has too often been a lamentable lack of integration in African history with Africa equated with Black Africa south of the Sahara and anglophone writers demonstrating a far too hazy appreciation of the sources in French and Arabic. These chapters stress the importance of North Africa in the history of the Mediterranean. Here Alexandria emerged as the most important commercial center while the Phoenicians reached their greatest power in Carthage.

Two matters concerning iron working are perhaps worth correcting. On page 359 it is stated that "early iron technology is not thought to have been able to produce tempered metal." Research in South, East, and West Africa indicates that the African smiths produced iron of such quality that there is talk of using the term "steel" rather than "iron" for the age in question. On page 331, in castigating Oliver Davies's 1000 B.C. date, Mauny has failed to take notice of Davies's 1973 full report on Ntereso in which he retracted that early date.

The inclusion of numerous figures and plates adds to the usefulness of the volume even though the pictures of the Zimbabwe ruins would have been more appropriate in volume 3, which covers the period of the main building of the Great Enclosure and the Acropolis walls. There is a lack of coverage of the East African coast and Madagascar for the period under review, and it should perhaps have been pointed out by the editor that they are included in volume 3. Though conventional, this is nevertheless a work of elegant scholarship and an invaluable reference tool.

MERRICK POSNANSKY  
*University of California,  
Los Angeles*

ADRIAN HASTINGS. *A History of African Christianity, 1950-1975*. (African Studies Series, number 26.) New York: Cambridge University Press. Cloth \$34.50, paper \$9.95.

African church history continues to attract scholarly attention precisely because organized Christianity remains an effective force on the continent. In recent years attention has been given to the state

of Christianity in Africa since the attainment of independence; the book under review is of this genre.

The study is divided into four major chronological periods—1950, 1951–58, 1959–66, 1967–75. Within this framework, Adrian Hastings discusses three major themes: (1) the relationship between the church and the state, (2) the operations of the mission churches, and (3) the African independent churches. Hastings accurately observes that the colonial era was clearly the age par excellence of the expatriate missionary. Not only did European missionary presence predominate in practically all the mission churches, but also these same missionaries tended to run the churches as colonial estates. The close identity of interest between the churches and the colonial state was reflected in the attitude of the former toward African nationalists. Indeed, "the churches tended so strongly to disapprove of the independent expression of political views by their members" (p. 31), thus creating the impression that they were antagonistic toward African political emancipation.

The Africanization and indigenization of the mission churches receive significant treatment in this book. Many churches, particularly the Roman Catholic Church, deliberately resisted the training of a significant number of local priests and so gave the Africans cause to believe that they had come to Africa not merely to serve but to dominate. By 1950 there was only one black Catholic bishop in all of Black Africa—Bishop Joseph Kiwanuka of Uganda. Others were elevated much later, especially after independence. Take the case of Nigeria. At independence in 1960 there were nineteen bishops in the country. Of these only four were Nigerians. Today, there are thirty bishops of whom twenty-six are Nigerians. In most of the other African countries, rapid Africanization is in progress. Africanization has invariably removed expatriate domination and overwhelming racial arrogance (pp. 46, 117).

Since independence, the mission churches have faced challenges on several fronts. First, there is the increasing demand for speedier liturgical adaptation to make African Christianity a distinctly African institution, adapted to the African milieu. The independent churches may reflect more of the African situation than the mission churches, hence their increasing popularity. Another area of concern to the church is the increasing secularization of the nation-states. Demand for more authentic indigenization by some nationalist leaders has taken the appearance of frontal war against Christianity. President Mobutu's challenge to the Catholic Church in Zaire in the early 1970s is a case in point. "We are now embarking on our cultural liberation," Mobutu declared, "the reconquest of our African, Zairian soul" (p. 191). Certainly Mobutu's

search for "authenticity" posed a problem to the Christian church in Zaire just as Nkrumaism did in Ghana in the 1950s and 1960s.

All in all, this is a very informative book, a significant contribution to Africanist literature. Specialists, especially those concerned with African church history, will find it informative and valuable. Though based largely on published works, it is both comprehensive and balanced. The author's assessment of the role of the church, especially in the southern part of Africa, where liberation movements predominate, is instructive. Granted that the churches have in one form or another criticized the injustices and racialisms practiced in this region, "the overall achievement remained limited, both on account of the ambiguity of the church's own internal practice, and because of a basic inability of the bishops to face up to the inherent moral rights of the liberation movement challenging a ruthless and oppressive minority government which has refused for years all peaceful change." Africans, Hastings concludes, are "looking for deeds not words, for a major change of law not a well worded protest" (pp. 216-17).

FELIX K. EKECHI  
Kent State University

L. H. GANN and PETER DUIGNAN. *The Rulers of Belgian Africa, 1884-1914*. Princeton: Princeton University Press, for the Hoover Institution. 1979. Pp. xv, 265. \$17.50.

Lewis H. Gann and Peter Duignan are best known as the editors of a massive, five-volume study of *Colonialism in Africa, 1870-1960* (1969-73) and of *African Proconsuls: European Governors in Africa* (1978). They also co-authored a militant apologia for Western colonialism in sub-Saharan Africa (*Burden of Empire* [1967]) and more recently have devoted themselves to a series of parallel studies of pre-World War I colonial rule as part of the Hoover Institution's "Builders of Empire" project. The present volume is a companion to *The Rulers of German Africa, 1884-1914* and to *The Rulers of British Africa, 1870-1914*, but it is not likely to add much to the two authors' fame.

Those who are acquainted with Gann and Duignan's earlier writings will hardly be surprised to find them once again engaged in their familiar activity of justifying Western colonialism. The present volume conforms to what the authors complacently call their "unfashionable" approach, which consists of arguing (*pace* Marx, Engels, Livingstone, and Kipling) that European colonization "played a progressive as well as a negative role in the backward regions of the Victorian world" (p. xi). Like other allegedly "revisionist" views of the neoconservative

school, this line of argument will only comfort those who passionately need to believe that, whether it be in Indochina, Chile, Iran, or Africa, the West is necessarily on the side of the angels.

One may legitimately wonder what new perspectives can be added to Gann and Duignan's case by the study of Belgian rule from 1884 to 1914. The book may serve some useful purpose by pointing out that Belgian colonial rule was not substantially different from that of other imperialists, or by documenting the fact that Belgian colonialists were neither worse nor better than their British or German counterparts. The choice of the 1884-1914 time frame, which makes obvious sense in the case of the German, and even of the British, territories, is singularly inappropriate for the Congo, however. On the outbreak of World War I, less than six years after the 1908 *reprise*, direct Belgian rule had barely begun, and it was not until the following decade that the Belgian colonial system developed the distinctive features that it was to retain until the 1950s. Whether in terms of socioeconomic change, of native administration, or of the specific coalition of interest groups that presided over Belgian colonial policy, the year 1914 represents neither the beginning nor the end of an era.

As a study of either the Leopoldian or of the Belgian systems, therefore, the book falls short of earlier works that concentrate more exclusively on one or the other, such as Ruth Slade's *King Leopold's Congo* (1962) or Roger Anstey's *King Leopold's Legacy* (1966). Gann and Duignan offer valuable insights into the social backgrounds and motivations of the soldiers and officials of the early colonial period. The treatment of this material is far from systematic, however, and consists for the most part of impressionistic vignettes that will add little to the information of those already familiar with Belgian sources. Descriptions of the Free State administration and of the post-1908 structure of Belgian rule (later to be substantially altered) will also be of some use for nonspecialists.

Aside from these limited merits, however, the book suffers from such an accumulation of factual errors as to jeopardize its value as a reference work. The list of such errors, which ranges from inaccurate geographic data to misinterpretations of existing information on Belgian and African societies, would be too long to detail. When added to the occasional contradictions, to the discrepancies between statistical data, and to startling naïvetés (the downfall of the Congo "Arabs" is attributed, *inter alia*, to their "lack of seapower"), not to mention misquoted sources and typos, they achieve a level of carelessness that one had not been led to expect from books published under the Princeton imprint.

Paradoxically, and for all their proliferation, such

errors do not significantly detract from the cogency of the authors' thesis, which rests only marginally on facts and derives its momentum from ideological postulates that can only be accepted or rejected *in toto*. Theirs is a vision of colonialism that owes its appeal to emotional rather than rational arguments. Except for a brief conclusion, this vision is not, in point of fact, developed at any great length, since most of it has been presented in Gann and Duignan's earlier writings, and the authors are, for the most part, content to justify colonialism in general—and Belgian colonialism in particular—through such oblique references as their approving quotation of "journalist-historian" George Martelli's smug (and inaccurate) remark that "every building in the Congo more than six feet high was put there by the Belgians" (p. 219); their casual dismissal of the Mahdist uprising in the Sudan as "an exploitative theocracy misnamed a revolutionary state by its latter-day admirers" (p. 57); or their matter-of-fact characterization of the Africans who resisted Leopold's brutal conquest as "an enemy who would use the most refined methods of torture imaginable" (p. 53). Adding to the emotional charge are the occasional parallels (quite out of context, as a rule) between colonial violence and twentieth-century counterinsurgency campaigns, in which Belgian colonial soldiers are successively described as "counterguerrillas by profession" (p. 58) and then equated by virtue of their backgrounds with "the most famous partisan leaders of our time" (p. 65).

The disappointment one experiences when reading Gann and Duignan's latest work is mitigated by the fact that competent and scholarly studies of Belgian colonial rule already exist. As for their reappraisal of Belgian colonialism, it will only persuade those who are already convinced of the merits of "benevolent imperialism."

EDOUARD BUSTIN  
Boston University

ADAMU MOHAMMED FIKA. *The Kano Civil War and British Over-rule, 1882-1940*. New York: Oxford University Press. 1978. Pp. xvii, 307. \$33.00.

This book will be of value to those scholars interested in African history, Islam, imperialism, and the impact of colonial rule. Kano is one of the two or three largest cities in Nigeria, and, in the nineteenth century, it was the capital of the most prosperous emirate in the Sokoto Caliphate, which itself was the largest state in nineteenth-century Africa. Until now, there has been no general history of Kano, and this lack has affected the reconstruction of the whole history of this empire. Because of the size of its population and its economic potential, the emi-

rate was one of the more important prizes in the scramble for Africa. The British conquest in 1903 introduced colonialism here much later than for most of Africa. Nonetheless, the policies introduced at Kano had far-reaching consequences as a prime example of Frederick Lugard's theories of "indirect rule." Adamu Mohammed Fika's exploration of the events leading up to the conquest, especially the civil war that shook Kano in 1893-94, and his examination of the impact of British rule make this book a significant contribution to the field. Fika's presentation of a point of view that hardly accepts the desirability or inevitability of colonialism adds balance to this area of history, which even now is heavily weighted in favor of apologists for colonialism.

The civil war raises interesting questions about the history of the Sokoto Caliphate. The failure of the Sokoto government to impose its candidate for succession to the Kano throne demonstrates the limits of centralized authority. The fact that the successful candidate secured support from nominal enemies who were not part of the caliphate highlights the diffused nature of power. By contrast, the British regime, as unorthodox in its imposition as the independent governments of 1893 and 1894, was much more powerful. Fika's examination of this transition is perceptive. He shows how British rule increased the authority of the central government at the expense of titled officials, both free and slave. Colonial aims were very narrow—rationalization of taxation, elimination of slave officials, prevention of social unrest, and abolition of slave trading. The only economic measure of importance was the construction of a narrow-gauge railway to Kano, completed in 1911. The issue of slavery was treated lightly.

Fika's methodology is one that is now standard in the study of African history—a combination of archival sources, Arabic literature, published European accounts, and interviews that the author conducted in Kano. Indeed the documentation is impeccable. One might wish that the interviews had been tape recorded and deposited at archives for the use of scholars, but there is no question about the standards employed in gathering the material. There is much new information here, and consequently much of the history of Kano is clearer by far than has hitherto been the case.

The only weakness in the book is the treatment of the economy. The *laissez-faire* policies of the British limit the value of colonial documents, while a reliance on interviews with aristocrats invariably prevents a full understanding of merchant, peasant, and slave activities. These are topics for others, however. Fika's contribution explores official behavior—both British and Fulani. The analysis of taxation, aristocratic alliances, the role of slave offi-

cials, the response to British aggression, and similar topics is a major contribution to the study of the Sokoto Caliphate and British Nigeria.

PAUL E. LOVEJOY  
York University

JOHN D. HARGREAVES. *The End of Colonial Rule in West Africa: Essays in Contemporary History*. New York: Barnes and Noble. 1979. Pp. xvi, 141. \$23.75.

John D. Hargreaves of Aberdeen University is well known for his important volumes on the late nineteenth-century European partition of West Africa and his brief interpretive history of the states of former French West Africa. In the four essays that form the core of this volume he examines several aspects of political decolonization in the Gold Coast, Nigeria, and Sierra Leone during the 1940s. References to the French territories are limited to Senegal, Ivory Coast, and Guinea, plus the trusteeship territory of Cameroun, which is usually considered part of Equatorial Africa or Central Africa. They serve primarily as comparisons with the experiences in the British territories. At the same time, references to the 1950s are chiefly retrospective. They look back to the pre-1939 origins of the transfer of power rather than ahead to the final stages that culminate in independence in the late 1950s and early 1960s.

One of the essays, which treats the historical backgrounds, appeared as a booklet in 1976. Two others are scheduled to appear in 1980 in their original form in volumes edited by Prosser Gifford and William Roger Louis on transfer of power in Africa (1945-65) and by W. H. Morris-Jones and Georges Fischer on the British and French experiences with decolonization and its aftermath; one of them deals with the formation of British policy during and right after the Second World War and the other with decolonization in Sierra Leone through 1951. Only the fourth essay, which discusses the rise and course of African nationalist movements, particularly their collaborative role in the decolonizing process, was specifically composed for this collection. In all but the first essay the author utilizes recently opened archives from the British Colonial Office. He enriches them with personal insights from his involvement with Africa during three decades, including a period as a professor at Fourah Bay College, Sierra Leone, in the early 1950s. Taken as a whole, his essays show that British policy formulated in wartime mainly in the light of interwar thinking had to shift priorities from long-term economic and social development to an accelerated transfer of power in the wake of the decolonizing forces generated by the conflict. They show how the British largely retained political initiatives

by collaboration with the new urban-based African movements that had succeeded in establishing ties with the countryside. They place the developments of the 1940s in a context that reaches back to the onset of the colonial period.

The three shorter essays that make up the last quarter of the text deal with the author's experiences in Sierra Leone, British colonial universities, and the relationship between African and contemporary history. They are delightfully provocative because of their perspicacity and wit.

DAVID E. GARDINIER  
Marquette University

TESHOME G. WAGAW. *Education in Ethiopia: Prospect and Retrospect*. Ann Arbor: University of Michigan Press. 1979. Pp. xv, 256. \$16.50.

Compared to what this book might have been—stimulating, provocative, insightful—it is disappointing. Supposedly “an expository analysis of the development of education in Ethiopia from the early part of the Christian era to the present” (p. ix), this work treats Ethiopia's first eighteen centuries in twelve pages, arriving at the “Foundations of Modern Education” in chapter 2. Teshome G. Wagaw practically ignores Ethiopia's fascinating system of traditional church education, while deeming mere mention sufficient for less formal Moslem, Jewish, and animistic structures. Wagaw discusses neither socialization nor the values of Christian Ethiopian society, material that would undoubtedly clarify early church and parental resistance to modern education and the recurring failure to attract sufficient females into the system.

Wagaw is not uncritical of his country's educational progress, but his conclusions are hardly astounding. Despite considerable imperial attention, pre-1935 educational development was, at most, modest; neither did progress following the Italian war leave much room for back-patting. Educational programs were generally improvised, mismanaged, and wasteful. Thus, after a generation of effort, Ethiopia, a nation proud of its heritage as a leader of African independence, was shocked to discover in 1961 that it was among the continent's most educationally backward countries. Redoubled efforts largely failed, so that by 1970 nongovernmental schools were doing relatively more to meet the nation's needs. Wagaw seems content with gentle admonition: “. . . even within existing limitations, more progress and fundamental change could have been achieved” (p. 196).

Although the author charts statistical progress (student-teacher ratios, the number of classroom units, the levels of expenditure), he never really penetrates the heart of Ethiopia's educational diffi-



culties. Wagaw clearly prefers to side-step controversial political developments. Eritrea provides one interesting example, as an area that progressed substantially under the 1950s federation, mostly at its own expense, only to see success falter in the 1960s as part of Ethiopia's underdeveloped periphery.

Wagaw hardly mentions the growing political turmoil in the country from the abortive 1960 coup onward. One wonders how these disturbances affected academic quality and whether the emperor's commitment to education was consequently re-evaluated. The controversial Sector Review of the early 1970s can hardly be separated from the developing revolution. Like its predecessors, this plan was mismanaged and largely idealistic. Was the government's decision to implement it honest or calculated? Certainly funding for such an ambitious undertaking was indefinite, and resistance from an aspiring middle class, already frustrated by lack of advancement and opportunity, was, surely, to be expected. Wagaw has little to say on these questions.

One last point. The book contains a bibliography of over eight hundred individual entries but only half as many footnotes. One wonders how the remaining materials were employed, why this section was not "abridged" in line with the text. Apparently, the University of Michigan Press is incredibly short of publishable material.

CHARLES W. MCCLELLAN  
State University of New York,  
Plattsburgh

CHARLES PERRINGS. *Black Mineworkers in Central Africa: Industrial Strategies and the Evolution of an African Proletariat in the Copperbelt, 1911-41*. New York: Africana Publishing, 1979. Pp. xvii, 302. \$35.00.

Can one "concretize a theory of proletarianism"? Following the path of Central African historical revision, Charles Perrings tries. He shows how the labor needs of the copper mines of Katanga and Northern Rhodesia, set in an overall economic framework based on complex, differing attitudes toward the structural attributes of indigenous employment, led to the displacement of migrancy by stabilization as the predominant mode of worker utilization. He goes on to conclude, with the kind of logical leap that characterizes so much of this genre, that stabilization established the preconditions for proletarianization.

Perrings also sees the beginnings of proletarianization in the security that came to African miners in the 1920s (primarily in Katanga) when they were on the favorable side of the demand-supply equation. Later, in Northern Rhodesia, Perrings be-

lieves that by the early 1940s Africans had achieved proletarianization: their attitudes toward employers had changed; their attitudes toward their conditions of employment had altered; and, subjectively, they were proletarian. What is never clear is the extent to which being proletarian—if it ever existed—made any difference to the lives of the Africans involved.

One fears that Perrings, like some of his immediate predecessors, has sought evidence for models and constructs that are at best, given the data available, difficult to apply to Central Africa. Moreover, it is conceivable that theoretical notions derived from a very different European context cannot blithely be used to analyze Africa. Perrings and several of his predecessors remind us, albeit unwittingly, how important it is first to understand Africans on their own terms and in the context of what, in this case, would mean their own response to the opportunity to act in a proletarian fashion.

Perrings has understood these conceptual caveats far more fully than his predecessors. Packaged between each chapter's opening and closing attempts to be theoretical is a running comparative history of labor and capital in adjoining areas of copper exploitation during the formative years (1910-41) of the industry. The author shows with painstaking clarity how both the Katangan and Northern Rhodesian industries were established; how their labor recruitment policies altered in accord with greater supplies of labor; how they received and controlled the resultant labor forces; and in what degrees and why the Central African mines (unlike the gold mines of South Africa) ultimately depended upon a stabilized, married, trained cadre of black workers rather than short-term migrants.

The strengths of this book reside in the author's grasp of the ways in which the geological structures of the mining properties on both sides of the border imposed different economic imperatives on the employers; in his emphasis on the differential impact on labor supply of discouraging (Northern Rhodesia) and not discouraging (Katanga) effective African agricultural entrepreneurial activity; and in his delineation of the ways in which various Northern Rhodesian ethnic groups responded to industrial opportunity.

Perrings's analysis of the etiology of the 1935, 1940, and 1941 strikes is unconvincing. In these cases, unlike much of the rest of the book, his conclusions outrun the evidence. He ends his analysis arbitrarily in 1941 (when the proof of his thesis is surely to be found in the 1940s and 1950s). He pays little attention to life in the compounds and is overly dependent upon a narrow range of published and unpublished data.

ROBERT I. ROTBERG  
Massachusetts Institute of Technology



THOMAS H. HENRIKSEN. *Mozambique: A History*. London: Rex Collings; distributed by Rowman and Littlefield, Totowa, N.J. 1978. Pp. 276. \$22.50.

Thomas H. Henriksen has the distinction of having published the first history of Mozambique ever to be written. It must certainly have been a daunting task to try in 232 pages to do what no one has attempted before, and the result is inevitably something of a "curate's egg." The most successful part of the book, and the part that contains the author's own research, is the last four chapters describing the origins of FRELIMO and the war of liberation. A lot has been written about FRELIMO, most of it political myth making, and Henriksen has tried to present an account of the movement, its successes and failures, that avoids the excesses of political commitment of other writers. It is a balanced and very sensible account that is probably as good as the present state of available information allows.

FRELIMO was unusual among liberation movements. It was formed abroad and had no roots inside the country. Indeed, virtually nothing is known about protest movements inside Mozambique. After the murder of Mondlane it was dominated by its military wing. Its history has been beset by defections and personal and ethnic hatreds, yet no rival to it has ever succeeded in establishing itself. This is surprising for, as Henriksen points out, FRELIMO was always coldly received by the country's biggest ethnic group, the Makua, and made virtually no progress among the people of the *Sul do Save* region. FRELIMO still remains something of an enigma. At one level it was clearly an effective military and civil organization capable of mastering large areas of the country and organizing guerrilla campaigns on numerous fronts. On the other hand its successes after ten years were limited; it did not succeed in interfering with the Cabora Bassa dam; it had not penetrated any of the urban areas nor won over the population in at least half the country; it was taken by surprise by the Portuguese collapse in 1974; and the history of Mozambique since is clear evidence that the implications of victory had not been thought through.

These four chapters can be recommended as a good, up-to-date summary of the war of liberation, but the same cannot be said for the rest of the book. Though the author refers to many Portuguese works, the early chapters are principally summaries of well-known and easily available works in English. His coverage of these is far from complete, and there are no references to many of the theses written on Mozambique in the last fifteen years—for instance, those by Hafkin, Hedges, Alpers, Langworthy, Phiri, Schoffeleers, Mudenge, and Bhila.

There are other limitations to this first section as

a scholarly contribution to African studies. There are factual mistakes, sufficiently numerous to require comment. For instance, on page 105 we are told that the Boror Company was a subsidiary of the Zambesia Company and that the Niassa Company's charter ended in 1926 and the Mozambique Company's in 1942. All these facts are wrong. Another drawback is the journalistic style. The use of cliché, slang, and journalese not only jars the ear but also can detract from the force of what the author is trying to say. For example, "Mwene Mutapa's empire arched high in a golden curve before descending to fragmentation" (p. 8); "He sailed the Portuguese imagination to the outer edges of Mercator's projection" (p. 23); "smash and dash raids" (p. 32); "This thumping victory became a foundation of sand" (p. 38); and many, many more.

It is significant that the really bad writing occurs only in the part of the book where the author is summarizing the work of others. This section is really very slight, and it is not easy to imagine for what audience it has been written. It is a pity that more room could not have been found to expand upon the sensible and important points the book has to make in the last four chapters.

M. D. D. NEWITT  
Exeter University

NORMAN ETHERINGTON. *Preachers, Peasants, and Politics in Southeast Africa, 1835-1880: African Christian Communities in Natal, Pondoland, and Zululand*. (Royal Historical Society, Studies in History Series, number 12.) London: The Society. 1978. Pp. xi, 230. \$18.70.

Despite the pleasing alliteration of its title, the subtitle of this book is the better guide to its contents and aim, namely, "to focus attention on the African response to Christianity in southeast Africa" (p. 4). The response of the overwhelming majority to European missionary activities was one of suspicion and resistance. In Natal missionaries could have uses—strengthening a tribe's position, assisting in relations with colonists, providing education and other services—but acceptance was limited by irrelevance and fear that Christianity would destroy traditional relations of kinship and mutual obligation. In light of the extensive tribal fragmentation in Natal, this "generalised hostility" (p. 55) was remarkable. Resistance outside the colony was still more effective. The use of African state power meant that by 1873 "the combination of official restrictions and popular resistance had brought missionary operations to a virtual standstill in Zululand" (p. 83); Cetshwayo's accession hardly marked a turn for the worse!

Missionaries' early optimism soon evaporated.

Some circumstances were clearly unfavorable—for example, Natal “lacked the fluid social and economic conditions which led, elsewhere in Africa, to competition between Christian and non-Christian chiefs” (p. 70). Moreover, missionary numbers and denominational variety “provided a range of choice . . . rarely open to would-be converts” elsewhere in Africa (p. 100), provoked competition for converts, and lessened missionary bargaining power. Except for Bishop Colenso, whose hopes of partnership between church and state were dashed by the Langa libalele affair, most missionaries matched Nguni resistance with their own steady conversion to imperialism and support for the Zulu War of 1879. Yet these years still saw the increase of the *kholwa* (believers), who numbered perhaps 10,000 by 1880.

If religious inducements to conversion were few, the ability of missions to provide employment, homes, and ultimately land, were crucial in attracting converts where insecurity was rife and “the homeless were numerous” (p. 92). Norman Etherington examines the origins of mission station residents and illustrates how their sense of isolation from both white and black neighbors encouraged material and educational progress. Their adaptation to colonial capitalism was rapid and successful until stifled by colonial controls after 1880—responses paralleling those charted in recent writing on the peasantry of southern Africa. A most interesting chapter on *kholwa* political and religious initiatives demonstrates their essential moderation, as evident in the *Unzondelelo* movement of 1875. It is argued that the new self-confidence of *kholwa* communities only inspired religious separatism and sustained political activity when their skills and ambitions were denied broader fulfillment by the sharp decline in secular opportunities open to Africans after 1880.

Using a wealth of material from American, European, and South African archives, Etherington writes well and relates his findings most constructively to the work of other historians like Sundkler and Ayande. His book is of considerable interest and use to historians both of Africa and nineteenth-century missionary enterprise.

ANDREW PORTER  
King's College,  
University of London

R. KENT RASMUSSEN. *Migrant Kingdom: Mzilikazi's Ndebele in South Africa*. London: Rex Collings. 1978. Pp. 262. \$17.50.

*Migrant Kingdom* is essentially a narrative of the first twenty years in the history of one of southern Af-

rica's most important nineteenth-century states, that of Mzilikazi's Ndebele. As such, it is a far different book from the one that R. Kent Rasmussen originally intended to write. He had assumed that the broad outline of Ndebele history was well known, since so much had been written about them. On closer examination, however, he concluded that “most of what has already been written on early Ndebele history is so erroneous that it is almost worthless” (p. 1). Thus, he took upon himself the task of getting the facts about Ndebele history straight and undertaking a thorough investigation of their existence in South Africa prior to their migration north of the Limpopo River in 1838–39.

Rasmussen divides his study into five chapters, each representing a critical phase of Ndebele history. In the first, he examines Mzilikazi's early life and concludes that he had no special relationship with the great Zulu leader, Shaka. The second chapter focuses on the formation of the Ndebele state. Most historians have the Ndebele settling in the eastern Transvaal only briefly and then migrating directly to the Pretoria region. Instead, the Ndebele moved continuously about the eastern Transvaal for approximately two years and then migrated to semipermanent settlements along the Vaal River, before finally moving to the area of Pretoria. This phase lasted from 1821 until 1827, not 1823 to 1825 as previously thought. Next, the author focuses on the Ndebele in the central Transvaal from 1827 to 1832, concluding that the extent of their military power and the geographical scale of their warfare has been considerably exaggerated. In the fourth chapter, dealing with the Ndebele in the western Transvaal during the years 1832–37, Rasmussen finds far fewer problems with earlier interpretations. This is largely because of the existence of accurate secondhand and firsthand accounts. The final chapter covers the best-remembered event in early Ndebele history, that of the 1838–39 migration to present-day Matabeleland, with the author again rectifying several prominent errors of fact.

Rasmussen demonstrates the firm grasp of relevant secondary and primary sources necessary for a study such as this. Extensive footnotes refer the reader to previous interpretations and Rasmussen's reinterpretation of the extant documentary evidence. He combines his knowledge of the source material with a clear organizational and writing style to produce a thorough and sound study. He thus has made a significant contribution to the historiography of nineteenth-century South Africa. From now on, *Migrant Kingdom* will be the starting point for those writing about the early history of the Ndebele and related peoples.

R. HUNT DAVIS, JR.  
University of Florida

## ASIA AND THE EAST

LOUIS ALLEN. *The End of the War in Asia*. Brooklyn Heights, N.Y.: Beekman/Esanu Publishers. 1979. Pp. xiii, 306. \$24.95.

Louis Allen, who served as a Japanese-language officer in the India-Burma theater of World War II, has written a number of books and articles on the military and diplomatic aspects of the Pacific War. In his latest work, Allen has attempted to provide an account of the human and political consequences flowing from the end of the war over a vast geographic expanse, covering Burma, Thailand, Indonesia, French Indo-China, the Indian National Army, Korea, Manchuria, and China. His book tries "to show the impact of [Japan's] surrender on the Japanese forces overseas . . . what problems it created for the returning Allies, what fears arose in the minds of the Allied prisoners-of-war as liberation came nearer, and most of all, what political changes it brought in its wake" (p. xii).

Using personal contacts that he established during the war and later renewed and expanded in addition to archival sources, Allen has explored the human dimension of the story with great skill. The admixture of a strident Japanese chauvinism and the naive idealism of pan-Asianism that often motivated Japan's wartime alliances with Asian nationalists is shown by Allen to have been based often more on emotional rather than rational bonds between the leaders involved. Its inner contradictions were quickly exposed toward the end of the war under the strains brought about by Japan's approaching defeat. The story of the personal betrayal and disappointment that this meant for many is told very well.

The author's treatment of the agony and suffering experienced by overseas Japanese settlers when they confronted the reality of Japan's shattered imperial design is often moving. His sketch of the plight of a Japanese settlement at Halahei, Manchuria, which vengeful Soviet troops and natives practically gave a bloodbath, is drawn with the feeling of a poet and the artistry of a craftsman. The dry statistics on war casualties can convey their poignant message only when they are "fleshed out" by such tragic episodes.

Allen's handling of the intricate political developments in Asia ensuing from Japan's defeat is, I am afraid, rather shallow. This may be because the author has attempted to cover far too many countries in one book. Much more detailed, sophisticated, and satisfying descriptions and analyses of the immediate postwar scene in Asian countries are available elsewhere. Allen is aware of many, though not all, of the other, more scholarly works on the subject, but, nevertheless, he seems to have convinced

himself that he could offer a book at once shorter and better! His description of the Indian National Army's ups and downs, for example, is appallingly brief and superficial. His analysis of the Korean political situation is, likewise, both brief and unfresh and shows no awareness of such fine works on the subject as Cho Soon-Sung's *Korea in World Politics*, (1967) and Frank Baldwin's *Without Parallel* (1974). Allen is informative and insightful on Korea only when he discusses the human problems stemming from the U.S. occupation's need for a temporary reliance on some Japanese bureaucrats in the teeth of Korean opposition.

It is because of his inadequate understanding of the political consequences of the war's end that Allen is able to issue such astonishing declarations as "the liberation of millions of people in Asia from their colonial past is Japan's lasting achievement" (p. 262). The distinction between forces that act as prime movers and those that act as catalysts was obviously lost on the author.

VIPAN CHANDRA  
Wheaton College

PAUL HENG-CHAO CH'EN. *Chinese Legal Tradition under the Mongols: The Code of 1291 as Reconstructed*. (Studies in East Asian Law, Harvard University.) Princeton: Princeton University Press. 1979. Pp. xix, 205. \$19.50.

In this work Paul Heng-Chao Ch'en has made a significant contribution to the limited body of material available in Western languages on Chinese law. The book falls naturally into two parts. Part 1 is an extended description of Yuan legal institutions, divided into sections on the development of Chinese Codes, the penal system, and the administration of justice. In the section on codes he has not only provided a detailed history of the codification movement but also some ingenious alternative interpretations of the interrelationships of different collections of laws. His sections on the penal and judicial systems are also by and large good. However, one problem does arise. Ch'en has set himself the task of making clear the distinctive legal achievements of the Yuan. Thus he compares the Yuan systems with the legal systems of other dynasties. Unfortunately, because so little has been published on earlier Chinese law, he singles out various Yuan usages as distinctive traits when they are in fact very similar to earlier Chinese practices. For example he says (p. 46) that the Yuan granted amnesties "more frequently" than did other dynasties when actually they granted them far less often than had dynasties like T'ang and Sung. On page 73 he implies that the Yuan was distinctive in that its prisons were merely holding facilities, not places where men were

being punished by imprisonment. In fact this was true in theory of all dynasties. In summary, Ch'en has provided an excellent description of Yuan practices. But his assertions that the Yuan system differed in specific ways from the systems of other dynasties must be treated with great care. Read in this light, part 1 can provide the student with the clearest picture of Yuan law available in any Western language.

Part 2 is a meticulous translation of Ch'en's reconstruction of the Chih-yuan New Code, a body of laws promulgated in 1291 and subsequently lost. The author has assembled from other Chinese sources ninety-six fragments he believes formed parts of the original work, grouping them under ten headings: public regulations, standard of selections, governing of the people, management of finances, taxes and corvee, taxes and levies, warehouses, construction and manufacturing, prevention of thefts, and investigation of cases. The Chinese text immediately follows. A careful comparison shows the consistently superb quality of the translation. This part might have been improved by giving content footnotes to such Chinese institutions as the "pastewall" and by using translations of technical terms (for example *ts'ao-tsei*) rather than romanizations, but these are minor blemishes on an outstanding translation. No doubt it will be widely used, as it deserves to be.

BRIAN E. MCKNIGHT  
University of Hawaii

HILARY J. BEATTIE *Land and Lineage in China: A Study of T'ung-Ch'eng County, Anhwei, in the Ming and Ching Dynasties*. (Cambridge Studies in Chinese History, Literature, and Institutions.) New York: Cambridge University Press. 1979. Pp. x, 208. \$27.50.

Among the most important contributions to the social history of late imperial China were the studies of the Chinese "gentry" by Chang Chung-li and Ho Ping-ti published in 1955-1962. In different ways, Chang and Ho stressed the need for each individual to pass highly competitive examinations to acquire gentry status and the great upward and downward mobility within the gentry stratum that resulted. In *Land and Lineage in China* Hilary J. Beattie challenges this view by looking at the Chinese ruling class as it existed at the local level. Her arguments are drawn from an essay by Chang Ying (1638-1708) about the importance of landholding and from her study of his home country in Anhwei.

In the most important section of the book, chapter 2, Beattie gives a history of T'ung-ch'eng county and its elite families from the Sung until the twentieth century. Drawing on three gazetteers dated 1490, 1696, and 1827, she is able to trace the pro-

gressive settlement and economic development of the county. She is also able to show that a large proportion of the eminent people of the county came from six families, all well-established by the end of the Ming, and all closely connected through marriage. Only a small number of the men in these families acquired degrees, but the social standing of the families remained high, based on wealth derived from landholding and on the education it made possible.

In the third chapter, Beattie pursues the complex issue of the economic returns from landholding. Her principal argument is that the tax burden placed on landlords by the government was not so heavy as to make land an unattractive investment. Besides discussing the changes in the tax system, she shows how landlords could protect their interests. For instance, she cites cases of leading figures in T'ung-ch'eng, such as retired officials, petitioning for decisions favorable to local landowners when various fiscal reforms were proposed or instituted.

For an area in central China, T'ung-ch'eng had an unusually large number of established lineages. Genealogies for almost seventy different kinship groups survive, nearly ten times the number for any other county in Anhwei. Most of these lineages were formed in the late sixteenth century, the first steps initiated by ambitious or successful men who wanted to enhance their families' prestige. The history of these lineages is discussed in the fourth chapter.

The central question here is whether lineage activities were a major factor in enabling ruling-class families to preserve their wealth and power or whether such activities merely enhanced men's prestige, in much the way that other charitable or cultural activities did. Beattie implies that lineages were very important, but the evidence she presents could just as well have been used to reach the opposite conclusion. As she shows, the corporate property of the lineages was small, less than that of a prosperous individual landlord. The activities of most lineages seem to have been limited to annual ancestral sacrifices, compilation of a genealogy every few decades, and, in some cases, provision of small subsidies to students. There is no evidence that upper-class members of the lineages were interested in their peasant kinsmen or ever organized them in order to further their interests vis-à-vis other local groups. And many lineages apparently had no eminent members. While Beattie presents fascinating evidence of the sort no other scholar has assembled, the full significance of lineages of local social structure still awaits further research.

*Land and Lineage in China* deserves a wide audience. It refocuses many of the issues in the debate about China's ruling class, and it provides a model of the kind of close study of particular localities that



is greatly needed if we are to advance our understanding of Chinese social history.

PATRICIA EBREY  
University of Illinois,  
Urbana-Champaign

SILAS H. L. WU. *Passage to Power: K'ang-hsi and His Heir Apparent, 1661-1722*. (Harvard East Asian Series, number 91.) Cambridge: Harvard University Press. 1979. Pp. xv, 252. \$22.50.

This book deals with the intricate succession crisis created by the aspiring sons of the K'ang-hsi emperor during the last three decades of his reign (1662-1722). The emperor deposed, redesignated, and redeposed his heir apparent, Yin-jeng, and from the chaos emerged the Yung-cheng emperor, one of the contenders. This was first covered in chapter six of Silas Wu's previous publication, *Communication and Imperial Control in China* (1970); the present work grew out of that chapter.

The *Passage to Power* is the first book-length study of the subject. Unlike other similar studies, it attempts a psychological analysis. This attempt is commendable because the succession struggle is by nature a good background for the psychoanalytic treatment. The book has benefited from some primary sources. While preparing it, Wu traveled to Taiwan, China. There he had access to the Ch'ing archives, notably the records on K'ang-hsi's daily official activities, housed in the National Palace Museum. This archival material is valuable to the study of Ch'ing history.

Wu's work contains some problems, however. For instance, he neglects the fact that Yin-jeng was in a defensive position throughout the struggle. As crown prince he became the common target of his ambitious brothers and was constantly challenged by them. To consolidate his status he had to counteract their challenges. The interplay of challenges and counterchallenges threw the entire court into confusion. This situation led K'ang-hsi to accuse him of many "crimes" and to dismiss him. Although he was the victim of the struggle, the throne's accusations were recorded as the official interpretation of the succession issue. Finally, from this interpretation appeared a variety of seemingly independent versions. Accepting at face value the official verdict and its derivatives, Wu fails to see the nature of the struggle, which remains as much of a mystery as ever.

Wu has a tendency toward dilation. His charges against Yin-jeng and his supporters of coup and assassination (pp. 1, 123, and 152) are not documented. Wu also overextends the sexual theory. According to him, for example, Buddhist temples were

the "sites of amorous adventures" (p. 91). For "pretty girls" and "fair-looking boys" Yin-jeng even set up "a fake river project" (p. 95). But none of these assertions are verifiable.

Wu's dilation reaches its climax with the story of Tohoci, a Manchu of the Joogiya clan and once the Peking police chief in support of Yin-jeng. By mistake or convenience Wu describes Tohoci as an imperial consort's brother, and from that background portrays Yin-jeng's addiction to sex and plots (pp. 149-150). These dilations result partly from Wu's faulty notation. Most errors happen with the citations from the *Veritable Records*. Altogether, nearly 10 percent of the 529 notes require a major overhaul. For instance, page 219 note 57 and page 222 note 27, and the first references in page 198 note 2 and page 208 note 12 are either inaccurate or incomplete. Sometimes the references are actually nonexistent books (p. 223 n. 45).

Most serious, some of Wu's major points are identical to those in my *Autocracy at Work* (1975), which devotes more than 30 pages to the succession problem. But nowhere does Wu's book cite or list it. For example, my book is the first study identifying Yin-chih as an active contestant after 1712 (pp. 71-75). Wu treats Yin-chih in the same way (pp. 165-67). Moreover, I conclude that "as early as 1707" Yin-chih tried to win imperial favor by inviting K'ang-hsi to feast in his garden (p. 74). Wu writes: "As early as 1707, K'ang-hsi had begun to accept invitations to visit Yin-chih's garden" (p. 165). I attribute the defeat of Yin-chih in the struggle to his "single-handed" approach and to his lack of "strong supporters" (p. 74). Wu also concludes "... he [Yin-chih] was in fact no more than a 'single-handed' faction, without a strong power base ..." (pp. 166-67). My book changes the first imperial son's name to "Yin-shih" (pp. 27, 313 n. 4), who had heretofore appeared as "Yin-t'i" in Western sources. Wu adopts the new name, too (p. 35). These examples, although one could give some more, sufficiently establish the reason for my grave concern.

PEI HUANG  
Youngstown State University

BRIAN HARRISON. *Waiting for China: The Anglo-Chinese College at Malacca, 1818-1843, and Early Nineteenth-Century Missions*. Hong Kong: Hong Kong University Press. 1979. Pp. xiv, 212. \$25.00.

Sixty years ago a biography of Robert Morrison, first Protestant missionary to China, could be subtitled "master builder" and aver in the preface that a fresh and promising chapter in the evangelization of China was about to start. No one talks that way today. The serial is over, the accounts in; the pre-



vailing verdict—it is certainly my students’—is that Christian missions were a piece of Western arrogance that deserved to fail and did.

Brian Harrison, professor emeritus at the University of British Columbia (and thus old enough to have once shared the earlier enthusiasm, did it suit him), proposes an alternative view. Morrison and his colleagues were not bigots. They were scholars and sensitive men. Through hard work they came to know a good deal about China, and (like the Jesuits of a previous age) they respected what they saw. They did not wish to ram Christ down Chinese throats. “Their aim was rather to set in motion a whole process of educational and cultural change . . . in the hope that new conditions favourable to the acceptance of Christianity would thereby be created . . .” (p. xiii). They wanted China Christian. But they expected conversion to occur naturally, as that isolated and straitjacketed society was gradually nudged into mankind’s common stream.

“Waiting for China” (as if in contradistinction to Jonathan Spence’s “To Change China”) is an alluring epithet, an attractive proposition. It offers us the possibility of rehabilitating simultaneously, as it were, Morrison, his successors, and—to the degree we are tarred with their brush—ourselves. It is impossible, then, not to be pleased with the thing as it is put to us, briefly in the opening pages of this slight volume, lengthily at the end. But the evidence for the proposition is slim.

It is drawn entirely from the short life of the Anglo-Chinese College at Malacca—and that institution does not seem to me to have amounted to much. Morrison, who reached China in 1807, built the college on Malaya’s west coast because he could not undertake it (or much else) at Canton. The Opium War lifted some China coast disabilities, whereupon the college moved to Hong Kong, changed its name and purpose, and effectively disappeared. Using chiefly the London Missionary Society’s archive, Harrison meticulously traces its history. What his text does not give us in names, dates, things published by the college press, his three appendixes do. In particular, we follow in detail the strained relations between the society and Morrison. The society thought the college should train Christians. Morrison was determined it should turn out educated men. That determination is almost the whole of Harrison’s case for the new Morrison, the “waiting for China” Morrison. It is not enough.

The proposition remains just that, a proposition, and the new Morrison is distinctly shadowy. Is Harrison himself really familiar with the man? Would you write of a person you knew well, an interpreter struck down by raging fever and stomach cramps at the height of a crisis that had exhausted him by its official demands, as Morrison was in 1834—would

you write simply that the poor fellow had “passed peacefully away” (p. 100)?

PETER WARD FAY

California Institute of Technology

N. M. KALIUZHNAIA. *Vosstanie ikhetuanei (1898–1901)* [The Boxer Rebellion (1898–1901)]. Moscow: Nauka. 1978. Pp. 362. 3 r. 20 k.

In June 1900 China shocked the world by declaring war on all foreign powers. It marked the culmination of the antiforeign movement led by a traditional type of secret society, I-ho-tuan, better known to Westerners as the Boxers. This outburst of popular frenzy with imperial patronage provoked the Allied siege of Peking and brought China nearly to national extinction. As a monumental historical event, with its far-reaching consequences, the I-ho-tuan Uprising has been a much-researched and interpreted topic. Collections of documents and memoirs as well as secondary studies abound in Chinese and Western languages. Now N. M. Kaliuzhnaia adds another tome to the growing number of monographs on this subject.

In her survey of existing historiography on I-ho-tuan, Kaliuzhnaia deplores that a study under true Leninist guidance is wanting and that Western bourgeois and Chinese historians share the tendency to overemphasize the imperialism of tsarist Russia in the event. Relying on a large corpus of source materials, she gives a conventional description of the causes of social unrest in China at the end of the nineteenth century and the origin of the I-ho-tuan. She then presents a phase-by-phase account of the uprising as the I-ho-tuan swept across northern provinces of China and into Manchuria. In analyzing the different components in the ranks of insurgents, she points out the role of conservative gentry and literati in guiding the movement. But her discussion of the bourgeoisie in China, which was represented by groups of reformers and revolutionaries, hangs loosely in the flow of events. She makes a fairly convincing, though not very original, point in asserting that the official reaction to the I-ho-tuan often reflected the local balance of political and social forces. When it comes to the crucial issue of Russian invasion and occupation of Manchuria, she glosses over key developments and obfuscates the issue with desultory vignettes on the insurgents. The Allied siege of Peking and the protocol signed at China’s defeat are covered in haste as she approaches the end of her rambling narrative. She concludes that the I-ho-tuan Uprising was doomed to failure because feudal China was as yet too backward to withstand the onslaught of capitalist countries and that after the catastrophe the Chinese people began to understand the futility of freeing

themselves from imperialist bondage under the Manchu regime.

This garbled account of the uprising and a theme that has become trite could still be tolerable if the book provided some gems of the rich Russian documentation of this period that might throw new light on the movement. However, the paucity of Russian sources, except lengthy verbatim quotations of Lenin, used in this monograph is disappointing. Materials from the Central Government Historical Archive (Leningrad) are occasionally quoted to footnote minor points. For example, the meager wage given to Chinese workers in construction of the Chinese Eastern Railway is cited as an evidence of the imperialist exploitation of Chinese masses while motives behind building the railway are not discussed. It is also frustrating to read archival citations that contain only the classification number without reference to the agency of the source or the context in which the information was recorded.

In short, *Vosstanie Ikhetuani* may be a useful textbook for Russian readers who find a good deal of factual information and synthesis of secondary studies on the I-ho-tuan, couched in Leninist phraseology familiar to them. For others, it does not enhance a better understanding of the movement from a different perspective nor with the use of new documentation.

NAILENE CHOU

Center for Chinese Research Materials,  
Washington, D.C.

ROBERT RUPEN. *How Mongolia Is Really Ruled: A Political History of the Mongolian People's Republic, 1900-1978*. (Histories of Ruling Communist Parties; Hoover Institution Publication, number 212.) Stanford: Hoover Institution Press. 1979. Pp. 225. \$7.95.

Robert Rupen, a professor of political science at the University of North Carolina, whose main fields are Russian government, Sino-Soviet relations, and U.S. national defense, is more widely known for his work on Mongolia. This most recent book is a summary and update of the neglected revolution of 1921, the first Communist revolution in Asia, from which the Mongolian People's Republic (MPR) emerged as the first satellite in the Soviet system and a prototype for those that later emerged in Eastern Europe.

Mongolia has been treated as a small country peripheral to everybody's concern. But now with new rumblings in Afghanistan, Iran, and other areas of Central and Inner Asia, it is imperative for more people to become better informed about the historical roots of contemporary problems. This study is

timely, also, in that it focuses on a strategic buffer state between the Soviet Union and the People's Republic of China.

Rupen demonstrates a commendable grasp of the facts and a generally detached approach. He does not idealize or romanticize the Mongolian revolution and its long, tortuous aftermath, and his critique in controversial areas is well documented. On balance, while he is sympathetic to the Mongols, he is critical of the overinvolvement of the Russians from the beginning and of the tragedies produced by revolutionary excesses of a Stalinist nature. The reader will look in vain for another volume that brings developments in Mongolia up to date with as much penetrating, critical analysis as Rupen.

The study is organized in three parts. Part 1, "Beginnings," includes an introduction, and covers the background from 1900 to 1920. Part 2, "The Age of Choibalsan," includes a discussion of the revolution and its aftermath, the period from 1929 to 1939 with its overshadowing influences of Stalin and Japan (until the latter's attack at Nomonkhan [Khalkhin-ghol]), and the period through World War II until the death of Choibalsan in 1952. The main contribution comes in part 3, "The Age of Tsedenbal," with sections on "China—Cooperation and Competition: 1952-1962," "China—Threat and Confrontation: 1963-1973," and a misnamed third section on "Developing Erdenet: 1974-1978." Larger than any of the three parts noted are the appendixes, notes, and bibliographies (87 pages).

While Rupen contributes insights to MPR political history, this is a very concise treatment, and one must still look to Charles R. Bawden, *The Modern History of Mongolia* (1968), for a fuller treatment of the earlier parts and to the *History of the Mongolian People's Republic*, translated and annotated by William A. Brown and Urgunge Onon (1976), for the extended, official Mongolian interpretation.

A dominant theme in the book, international relations, focuses on the problems of the triangular relationship of the MPR, the Soviet Union, and China. The role of Japan comes in for considerable discussion. The book is generally true to its design of being an analytical history of the role of the Communist Party in Mongolia, but, considering the nature of the story, it is not surprising that the narrative often comes out in terms of military history and power politics. To this reviewer, there seems to be an overabundance of facts and figures in the narrative. The interplay of nationalism, ethnicity, and Communism are highlighted throughout the book. Rupen stresses that the party has been the main instrument by which the USSR has maintained dominance in the Mongolian People's Republic, but the real inside story of how Mongolia is ruled is still not clear. We still only have circumstantial evidence as

to how Russian advisors manipulate and how Mongolian leaders arrive at decisions.

PAUL V. HYER  
Brigham Young University

J. W. DOWER. *Empire and Aftermath: Yoshida Shigeru and the Japanese Experience, 1878-1954*. (Harvard East Asian Monographs, number 84.) Cambridge: Council on East Asian Studies, Harvard University; distributed by Harvard University Press. 1979. Pp. xiii, 618. \$20.00.

Shigeru Yoshida (1878-1967) is one of the least known and most self-assured statesmen of the postwar era. As prime minister for most of the period from May 1946 to December 1954, Yoshida guided Japan through liberalizing reforms and economic recovery under American military occupation, and he was the chief Japanese architect of arrangements for regaining national autonomy in April 1952.

This provocative and thoroughly researched book, by John W. Dower of the University of Wisconsin, is no standard political biography. Instead, it is a set of stimulating essays on Japanese capitalism and imperialism, linked but not confined to the public life of a diplomatist-turned-politician who was at once both pragmatic and principled. If *Empire and Aftermath* sometimes does not bring Yoshida fully to life or detail the record of his five cabinets, this seems a small price to pay for its sharp insights into wartime and postwar Japanese policy making.

Although well-born and closely associated with the throne, Yoshida, like most other Japanese imperialists in the 1920s, worried about Chinese nationalism, communism, and Soviet expansionism because they menaced Japan's continental interests. Despite his image in the West as a liberal and an Anglophile, Yoshida lobbied quixotically in the 1930s to reconcile the British and Americans to the "active policy" (p. 65) he consistently favored in China. Capricious and impulsive in method, Yoshida was a monarchist to the core.

*Empire and Aftermath* skillfully describes the anti-military maneuverings of courtiers, senior statesmen, and other top elites to topple the cabinet of General Hideki Tōjō in July 1944 and press for a negotiated peace the following spring. As a leader of this group, Yoshida feared both insurrection from below (perhaps led by Communists) and revolution from above (displacing the key prewar elites by "fascist" military officers or—after 1945—by new rulers under the Americans).

When he became prime minister in 1946, Yoshida tried to perpetuate the national leadership and goals of the pre-1937 period. Since this aim conflicted with the early democratizing reforms of

the occupation, Yoshida worked especially hard to blunt the new rights enjoyed by labor and the Communist party. Dower argues persuasively that the occupation underwent a "reverse course," emphasizing industrial recovery and rearmament, in 1948. The Yoshida era correspondingly blossomed from this point forward.

But Yoshida proved to be no reactionary. He helped to harmonize "the old imperial consciousness and patterns of authority" with the ideals of the "democratic revolution" (p. 317) of 1945-1947. Distasteful as he found many of the reforms, Yoshida worked to remove their sting rather than wipe them out. Because he believed the country's fate was tied to a postwar *pax Americana*, he realistically committed Japan to a mutual security treaty with the United States, despite charges of a sellout to the Americans from both left and right. Yoshida accepted American bases in Japan "not so much to guarantee its security as to regain its sovereignty" (p. 377)—the signal achievement of a remarkable career.

THOMAS R. H. HAVENS  
Connecticut College

THOMAS R. METCALF. *Land, Landlords, and the British Raj: Northern India in the Nineteenth Century*. Berkeley and Los Angeles: University of California Press, for Center for South and Southeast Asia Studies, University of California, Berkeley. 1979. Pp. xiv, 436. \$22.50.

Western and Indian scholars of the high caliber of Thomas R. Metcalf have brought conclusively to light what has been wrong with Indian history writing up to recently. We simply did not have the facts. From Max Weber to Barrington Moore (*Social Origins*) and Gunnar Myrdal (*Asian Drama*), tempting generalizations about the Indian rural economy have floated into our literature—and, alas, our lectures—without much evidence to verify them. The "unchanging hierarchy of caste-bound control of land," the "transforming effects of British land policies," the "feudalistic zamindari system," and so on are attractive concepts that only the most meticulous researchers can effectively investigate. Metcalf has done it for the nineteenth-century central Ganges region, currently Uttar Pradesh state; it has been a work of some fifteen years, mostly carried on in the archives of Allahabad and Lucknow.

While this book's twelve chapters (arranged in three parts: pre-British, North-Western Provinces, Oudh), are smoothly written, some of them require close and repeated reading. The detail interrupts any attempt at rapid perusal. Readers unfamiliar with the lay of the land may scrutinize attentively each example cited under a general category and

become bogged down. Furthermore, Metcalf is cautious: he will not strive for meanings that go beyond the evidence. And the overwhelming impression that his chapters convey, particularly the early ones, is of the unmatched confusion of India's land-holding patterns. Occasionally Metcalf reports the lack of reliable statistics; for the rest he steers us manfully through a sea of treacherous diversity and contradiction, lands us briefly on an island of familiar meanings, and then plunges us back again. It was physically hazardous, he writes (p. 38), for the taluqdars (holders of estates) to venture into the countryside in the pre-British period. It is intellectually hazardous for us to do so now!

The book is about the taluqdars (zamindars of great holdings), on whom the British rested their control of the countryside and through whom they collected taxes from the villages. About half of the nearly four hundred pages sets those once-ruling proprietors into their post-Mutiny, mid-nineteenth century position; the remainder discusses their functioning as little more than landlords, intermediaries between the state and the farmers. As felicitously as possible, Metcalf juxtaposes the North-Western Provinces with Oudh, hoping thereby to elicit revealing comparisons. But in fact the effort results in two separate books, not one. In the North-Western Provinces, taken over one-half century before Oudh, British policies eased "the taluqdars down gently from their former preeminence" (p. 99) and replaced them with commercial zamindari classes of Brahmins and Banias. In Oudh, after a fling at breaking up taluqdari estates, the British decided after the 1857 rebellion to confirm that "gentry" class in permanent possession of their lands.

Metcalf is sympathetic to the taluqdars of Oudh. Within the limits set by their dependence on the British, they did fulfill their main function of managing usually vast estates. While noting their eccentricities, he avoids calling them rapacious or even exploitive (p. 307). They contributed to the preservation of old and cherished ways and upheld certain fused Hindu-Islamic cultural standards in the face of westernized urban leaders.

In assessing British policies, Metcalf describes their inconsistency: on the one hand they propped up the taluqdars as the focus of traditional loyalties and on the other hand tried to use them as English-style gentry to sponsor rural progress. It was left to independent India to cut through this dilemma and try to abolish the taluqdar-zamindar class. Both by accident and design, nineteenth-century rural society was transformed, if not upheaved, under British rule. Metcalf's patient probing of the often microscopic evidence reveals the many ways in which this happened.

CHARLES H. HEIMSATH  
American University

ROBERT ERIC FRYKENBERG, editor. *Land Tenure and Peasant in South Asia*. New Delhi: Orient Longman; distributed by Land Tenure Center, University of Wisconsin, Madison. 1977. Pp. x, 312. \$15.00.

The volume under review contains twelve essays ranging from analyses of early British land settlements in the late eighteenth and early nineteenth centuries to analyses of rural credit, farm mechanization, and land reform in India during the 1960s and 1970s. The authors range from historians (five), through political scientists (two), and economists (two), to a sociologist, an anthropologist, and a geographer. The editor's objective was "an anthology which could approach questions of land tenure from the perspectives of many disciplines" and that would "show agrarian problems as sets of symbiotic relationships" that "could not be dealt with effectively without reference to their entire past and present conditions." In the first respect the volume succeeds, and quite well: the contrasts among the various disciplines come out quite clearly. The effort to achieve symbiosis in holistic analyses is moderately successful, although the essays on British settlements do not go much beyond the analyses of fit and misfit between early British policies and the systems upon which the British impinged, with a revival of the old-now-become-new emphasis on privilege and exploitation. However, essays of nine to twenty-two pages (ten of the essays) hardly permit "holism" or reference to any "entire past and present." The closest one comes is the fifty-three page chapter by Hart and Herring on "Political Conditions for Land Reform: Kerala and Maharashtra": it is also one of the three most interesting essays in the volume.

Of the six strictly historical essays, four deal with south India, with more of the technical terminology of *inam* tenures than nearly anyone would want to know (Frykenberg may have earned the Heiden prize with nine technical terms and eleven caste names in eight lines of print). Adas's chapter on tenures of lower Burma is a good, short summary history, but the other essays among these six presuppose a fairly detailed knowledge of the history of early British Indian land policies and of the beliefs that have become common among scholars.

Perhaps because as an economic historian I am more economist than historian, I found the later essays more stimulating. Herring's "Land Tenure and Credit-Capital Tenure" neatly puts the proposition that the same conditions that give larger landowners disproportionate power in village life also give them disproportionate access to credit, with the two disproportions mutually reinforcing each other. Elder also neatly argues the plausibility (and does not try to argue a proof) that castes rising in the hierarchy align themselves with Brahmins of doubtful



authenticity who then validate the twice-born status of the rising caste and simultaneously validate their own status with the lands bestowed upon them. Hart and Herring (*supra*) make a strong case that some land reform and some decrease in inequality make further progress along these routes more difficult.

WALTER C. NEALE  
University of Tennessee

C. M. H. CLARK. *A History of Australia*. Volume 4, *The Earth Abideth For Ever, 1851-1888*. Carlton, Victoria: Melbourne University Press; distributed by International Scholarly Book Services, Forest Grove, Oreg. 1978. Pp. xv, 427. \$16.00.

This book again proves that Manning Clark is one of Australia's greatest but most controversial historians. This fourth of a five-volume *History of Australia* shatters many orthodoxies held to be self-evident by historians on the period 1851 to 1888. Clark arrived at his conclusions from a series of studies beginning in 1950, when he published *Select Documents in Australian History, 1788-1850*. In addition to making available both well- and little-known documents, he provided short, illuminating introductions to eight sections. In 1955 there appeared a larger collection of documents covering the period from 1851 to 1900, most never seen before, and supplied with longer and more original and insightful introductions. In 1957 the *World's Classics* series produced Clark's *Sources in Australian History*, repeating none of his previously published documents, yet traversing the beginnings to 1918. Encouraged by many, this reviewer included, to write a multi-volume history of Australia, Clark responded with his first volume in 1962. It aroused enormous controversy. He paused to produce a stimulating but more traditional *Short History of Australia* (1963), then returned to his magnum opus, producing volume 2 in 1968 and volume 3 in 1973. He admits weariness in his preface to this fourth volume, and closes the story in 1888 (the centenary of Australia's British foundation), not his projected 1900. God and his health willing, he will complete in retirement (from the nation's first professorship of Australian history) a fifth volume, and with it a much needed annotated bibliography, absent since volume one.

In the preface to volume 4, Clark writes that he is summing up the themes introduced in earlier volumes: "the influence of the spirit of the place on human behaviour, the struggle between classes for the ownership of wealth, the struggle for political power, and the confrontation between Catholic Christendom, Protestant Christianity and the Enlightenment." He is concerned "with the debate in

Australia about the life of man without God." The major theme, indeed, is "the impending breakdown of bourgeois society, succeeded by an age of ruin—which is still with us" (p. vii). He does not agree with A. G. L. Shaw that these years brought the beginnings of a high standard of living for the general population or with Russel Ward that there was a noble mateship on the frontier. He does not believe that the capitalistic builders and engineers softened Australia's harsh environment, and bush life to Clark is brutalizing, disheartening, and degrading. Clark includes more cultural history—analyses of writers (but alas too few painters and architects), churchmen, and educators—than any previous historian. Superb vignettes, some quite long, appear of such writers as Marcus Clarke, A. G. (Banjo) Paterson, Adam Lindsay Gordon, and above all Henry Lawson. There are equally superb accounts of such political figures as Sir Henry Parkes, Jack Robertson, and Sir Redmond Barry, and of the bushranger Ned Kelly. Volume 4 reveals Clark writing at his best. Drawing many a phrase and sentence from the King James Bible and the Anglican Book of Common Prayer and Hymnal, he attains the highest standards of narrative history. Where can one find a better account of the lives and works of Clarke, Lawson, or Gordon, or the ill-fated Burke and Wills expedition, or the eruptions and degradations of the larrikins? His insights into the role of women in a man's world, the Sydney *Bulletin*, the Salvation Army, and the suffering aborigines, to cite just a few diverse examples, are fascinating.

Although brilliant, the book is flawed. Details are not always correct. The prose turns an occasional purple. The methodology is sloppy, particularly the footnotes, which are unclearly cited and overloaded with the primary materials he knows so well, but which fail to cite some of the latest scholarship. An example is the omission of Geoffrey Blainey's *Triumph of the Nomads* (1975) when discussing aborigines. Clark scatters moral judgments on too lofty a scale. He plays the psychiatrist too often when probing the hearts of his characters. Despite unevenness and prejudice, however, Clark has almost completed a multivolume history of Australia that has asked and answered the really tough questions and is worthy of comparison with Bancroft's work in the grandeur of its design and execution.

SAMUEL CLYDE MCCULLOCH  
University of California,  
Irvine

W. G. MCMINN. *A Constitutional History of Australia*. New York: Oxford University Press. 1979. Pp. xiii, 213. \$29.95.



It seems useful to begin this review by remarking on how, with an Australian focus, W. G. McMinn selects and treats material within the genre of constitutional history. The book's language is parsimonious and its arguments are terse. This helps but does not overcome the problem of compressing nearly two centuries of material into two hundred textual pages.

The reader is guided through the fits and starts from which emerged, in what was originally a convict settlement, the advisory, representative, and responsible governmental institutions and the legal norms that, as of 1901, forged an Australian political entity. McMinn's historical craftsmanship is sound. While he notes trends and consequences, he quite properly discounts any serious interpretation that constitutional growth in the "colonial" period prior to 1901 deserves to be romanticized as a popular struggle for democracy or attributed to carefully tailored plans in London. Moreover, in keeping with his stated purpose of writing a study of the working and evolution of the Australian governmental system rather than a strict study of constitutional law, McMinn skillfully weaves together the influence of economic trends, social change, personalities, and fortuitous factors and the synthesis between Australian originality and borrowing from British, American, and other experience.

The less than half of the book devoted to post-federation development is in these respects less successful. There is rather less of a "holistic" treatment of constitutional growth, though in partial defense it can be argued that by 1901 an identifiable Australian community and ethos had already emerged. Understandably, much attention is paid by McMinn to judicial renderings and rationales, but the reader does not quite get a sense of Australian jurisprudence. McMinn is justified in stressing the movement and ordering of federal and state forces. It gives rise to his insistent thesis that the system has moved from balance to federal primacy to federal domination. McMinn does little with the political and judicial extension of Commonwealth external powers to sustain this thesis, following his allusion to the 1930s version of the theme. Moreover, he skims over the recent yet highly significant party-political and federal-state bargaining features of "new" or "cooperative" federalism. While the subject has not held the same prominence in Australian as it has in American society, more than casual mention of personal liberties and group rights would have been welcome.

In general, my reservations about the book refer much more to what McMinn only fleetingly discusses rather than to the quality of what he does discuss. Any reviewer will raise some questions of interpretation about any particular book. It should, however, be emphasized that while McMinn has

opinions, they are on balance informed and reasoned, and he is careful about being tendentious. An example is his explanation of the Governor-General's 1975 action in dismissing the Whitlam Labor Government. Furthermore, McMinn's presentation is not fitted into some special philosophical or partisan casing, and it is largely free from being argumentative. McMinn's work is not readily characterizable as radical, revisionist, economically or judicially behavior-determinist, or whatever. While it is unlikely to inspire heated controversy, it will serve as a reliable, workmanlike source.

HENRY S. ALBINSKI  
*Pennsylvania State University*

ROB PASCOE. *The Manufacture of Australian History*. New York: Oxford University Press. 1979. Pp. 207. \$28.50.

*The Manufacture of Australian History* is essentially a historiographical study of some fifty or so Australian historians whose primary interest lies in Australian domestic history and especially in the problem of Australian national identity. The Australian past, marked by an evolutionary weaning away from Britain in the growth to nationhood, rather than the revolutionary break of the American experience, has presented many Australian historians with the interesting task of creating a national image or identity by seeking answers to such questions as what made the transplanted Briton an Australian, just when the change occurred, and what brought it about.

Of course the focus of the study primarily on those historians who have confronted such issues bars from consideration those who have dealt with other matters than the shaping of the images of the national past. Further, it puts out of court much of the work of those who, while they have written on Australian themes, have also written on other subjects. Sir Keith Hancock's work in the British civil history series of World War II is unnoticed, as is his biography of Jan Smuts, though much time is given to measuring the influence of his seminal work, *Australia*. Those who might want a broader survey of Australian historiography should resort to J. M. Ward's essay in A. L. McLeod's *The Pattern of Australian Culture*.

In his consideration of the work of his various authors, Rob Pascoe has utilized the classification developed by the American philosopher Stephen C. Pepper. Pepper said that there were essentially four main approaches to the historical process: the mechanistic, the formist, the organicist, and the contextualist. These various conceptions have certainly not made any great penetration into American historiographical studies, although they have

been used and lucidly explained by Hayden White in *Metahistory: the Historical Imagination in Nineteenth Century Europe*. But where White used this classification of ideas in his consideration of some eight major figures in European historiography, Pascoe applies the various categories to a much greater number of historians and in a much shorter book. The terms of the ideas they convey thus become more labels than tools of intellectual analysis.

All this leaves the reader in some confusion. Ward had rather deprecated the idea that there were substantially different schools of interpretation of Australian history. Pascoe gives schools aplenty, for in addition to Pepper's system, he also employs more commonplace categories such as conservative, liberal nationalist, Old Left and New Left, and a few others beside. And indeed, it is really in the latter terms that Pascoe discusses the historians who interest him.

The author writes that the boot of the European has left a lasting imprint on the Australian landscape; one senses as well the strong imprint of the European Left on Australian historical writing. Pascoe devotes more effort to the so-called New Left than is perhaps justified. For however much they have rattled skeletons and led to controversy in the pages of Australian quarterlies, the actual production of votaries of that school is rather scanty, even with the inclusion of the works of the odd sociologist or two whom Pascoe admits to Clio's shrine.

DONALD C. GORDON  
University of Maryland,  
College Park

## UNITED STATES

HENRY SAVAGE, JR. *Discovering America, 1700-1875*. (New American Nation Series.) New York: Harper and Row. 1979. Pp. xvii, 394. Cloth, \$17.95; paper, \$6.95.

WAYNE FRANKLIN. *Discoverers, Explorers, Settlers: The Diligent Writers of Early America*. Chicago: University of Chicago Press. 1979. Pp. xiii, 252. \$15.00.

These two authors begin with the assumption, set forth on the dust jacket of Wayne Franklin's work: "that the discovery of the New World was not a single glorious act in 1492. It was instead a long process of progress and retreat . . . a process to which the power of the word was essential, and the diligence of the writers most basic." Henry Savage, Jr., deals with the physical America—the look of the lands beyond the horizon—and Franklin with the New World of the Mind.

The Savage book, one in the New American Nation series that has produced such notable works as

Billington's *The Far Western Frontier* (1956), is a useful but limited account of the "discovery" of America through the reports of explorers and naturalists. The assignment is extensive and produces awkward restrictions. The territory covered is the forty-eight contiguous states. The figures dealt with are men whose activities were widely reported in their time—thus ruling out, for example, Jedediah S. Smith's trailblazing travels. But there are substantial chapters on Lewis and Clark, Pike, the Astorians, Frémont, Powell, and outstanding naturalists such as Thomas Nuttall, John James Audubon, and Alexander Wilson. With a less comprehensive title, the author could have avoided the need to explain what he calls in his preface "the painful process of exclusion" (p. xvi), which has crowded out the Spanish in the Southwest and, to a lesser extent, the British and French. His sources are standard works, mainly dating before 1970; his chapter on Lewis and Clark would have benefited from the use of their *Letters* (1962), and the chapter on Astoria should not have been written without some reference to Kenneth W. Porter's essential *John Jacob Astor: Business Man* (1931).

Wayne Franklin has written an extended essay on the theme that "from the start language and event in America have been linked almost preternaturally to each other" (p. xi). He divides early travel books into three categories—discovery, exploration, and settlement—and confines his observations to a few representative but outstanding writers. Thomas Harriot's *A Briefe and True Report* (1588) is at once an account of past events and an assumption about the future of British America, a wide-angle view with a strong emphasis on anthropology. Antoine de la Mothe Cadillac, founder of Detroit, writes to his sponsors (1713) of his apprehensions about the infant colony of Louisiana. John and William Bartram travel the new land and publish their arresting observations on natural history (1751, 1791). Franklin suggests, with Crèvecoeur, that John Bartram's botanizing "began with a reaction against his own destructiveness—a guilt over white plantation" (p. 51).

Thomas Jefferson's *Notes on the State of Virginia* (1785) is properly identified as a work extolling the "regenerative powers of the New World scene" (p. 25), in refutation of the old canards of Buffon and other Europeans who saw nothing in the New World but degeneration or stasis.

Franklin is concerned with the preconceptions of New World travelers and the occasions when, not always inadvertently, they penetrated through some of the notions with which they came. "Like any other part of Old World order, language and its received forms—forms of literary art, but forms of discourse and syntax as well—were affected deeply by their transportation to America. Had Europeans

been almost wholly closed of mind, firmly set against any significant adaptations, it seems likely that the colonization of the West never would have succeeded" (p. 17).

A set of thirty-six plates illustrates Franklin's thesis that early artists controlled visual space in a way comparable to the control of verbal space by New World reporters.

DONALD JACKSON  
Colorado Springs, Colorado

LELAND M. ROTH. *A Concise History of American Architecture*. New York: Harper and Row. 1979. Pp. xxvi, 400. \$25.00.

In 359 pages of concise text with 304 apt and generally sharp illustrations, Leland M. Roth of the University of Oregon at Eugene offers a balanced and perceptive history of American architecture. A model textbook for students, it is also an amateur's delight, for the writer's style flows enjoyably, his comments are informative, his examples are telling and often fresh, and his illustrations appear on the pages where the illustrated buildings are discussed. Even where one senses that the format and text evolved from a semester's lectures with slides, the reader also respects the seasoned scholarship and long gestation of themes evident throughout this well-constructed and well-documented history.

Roth starts his story with the American landscape and native builders, then devotes the second chapter to colonial transplantations and his third to post-Revolutionary architecture ending in 1820; he describes the lure of the past and promises of the future in 1820-65 and, remarkably and rightly, carries the period 1865-85 in a separate chapter, "Age of Enterprise." He discusses urbanism and the varied architectural responses in his sixth chapter, 1885-1915, and offers fine insight into "Dichotomy: Tradition and Avant Garde" in his chapter on 1915-40. The eighth chapter, 1940-70, and the epilogue, "pluralism in the 1970s," will attract the attention of scholars already familiar with the earlier history.

The fine exposition and sure judgment throughout this book far outweigh its few instances of superficial ideas or aberrant criticism. I doubt that urban plazas are "anomalies in American cities" because they embarrass "the Protestant work ethic" (p. 324). I doubt that the architectural story of 1960-80 can be told without considerable attention to the giant corporate real estate financier developers, including Zeckendorf. Surely it is not informative to conclude the chapter, 1940-70, with a sentence that purports to explain "the enlargement of scope of the architect's domain" (p. 332) but does not and, in fact, refers merely to a greater "com-

plexity" and "playfulness." More disserving still is the concluding epilogue where, after flitting about the styles of Rudolph, Roche and Dinkeloo, Johnson, Stubbins, Eisenman, Meier, Venturi, Portman, Pei, and many other fashionable architects—omitting Thompson, Netsch, Jacobsen, and others who are less heralded now but whom history may celebrate—Roth refers approvingly to the "whimsical classicism" of the 1978 model for the AT&T headquarters and quotes Philip Johnson's remark about "a new uncertain, unclear, but clearly fascinating future" (p. 359).

Such dalliance with style distracts Roth from the very substantial narrative he had been telling for eight chapters, including the epilogue: the story of American efforts to build cities and towns. In fact, his epilogue is especially insightful in discussing recent contributions to public space and urban contexts, a theme he had introduced deftly in earlier chapters. To allow the posturings of mannerist scene makers to distract him from the great urban contributions of contemporary architects is out of character with Roth's earlier chapters.

Errors in name, spelling, or fact on pages 39, 88, 127, 274, 279, and 348 are readily corrected by the reader without diminishing his respect for a fine, enjoyable, and concise history of American architecture.

ALBERT BUSH-BROWN  
Long Island University

JAN COHN. *The Palace or the Poorhouse: The American House as a Cultural Symbol*. East Lansing: Michigan State University Press. 1979. Pp. xii, 267. \$15.00.

Increasingly nowadays the symbolic and metaphorical content of architecture, neglected for several generations in favor of formalistic concerns, is being rediscovered. An important milestone in this process is the appearance of Jan Cohn's *The Palace or the Poorhouse: The American House as Cultural Symbol*. Cohn's fundamental viewpoint is literary; the first part of this study examines how houses have functioned as symbols in literature. A mass of examples from the very earliest settlements in Massachusetts, Virginia, and Georgia down through nineteenth- and into twentieth-century novels conclusively shows that, no matter how compulsively leaders of the reigning avant-garde Establishment dictating architectural education insisted that no other concern than functional should determine building forms, in fact the mass of the population continued to perceive a symbolic element in architecture, to value architecture for metaphor and symbolism above all other considerations. In the process a wealth of new historical insight has been revealed. No effective study of American culture can be

made, henceforth, without reference to the kind of evidence Cohn shows to be inherent in architecture. Even more significantly, however, Cohn (who has long been prominent in the Popular Culture Association) has demonstrated how this traditional perception of how architecture works in and for society still lives on at the popular and commercial level. Her thesis builds to a climax, so that in some respects the last few pages constitute the heart of her argument. Talking about the architecture of Levittown, she observes that, "Like the houses inspired by the Colonial Revival of the 1870s, the Cape Cods and Colonials of Levittown have an aura of tradition and association. Although a family may buy such a house planning to inhabit it for as few as five years, the historical styling politely ignores their transience and provides an architectural symbolism that speaks of stability and permanence."

This, ultimately, is what house-building has always been about. It is the most visible symbol of having arrived at a fixed place in society, and so functions today on the popular and commercial level: "In many ways the American house has become more symbol than reality. The individual house is often no more than one in a series of houses; yet it assumes to itself the values once accorded only the ancestral house, establishing itself as the temporary representation of the ideal permanent home. These houses, furthermore, stand for property, despite the fact that most are held by banks to whom the residents pay mortgages. These serially-owned, bank-held houses are, with increasing frequency, the unindividualized results of mass-production and design. Yet, a man's house is still considered the expression of his own taste and character, and men and women find in their homes the greatest opportunity to express their personal taste. The activities of the house, as well, have changed. Home industries have long since vanished and home functions of child-rearing, education, and care for the sick and elderly have passed to public institutions. Only laundry and food preparation remain, and should they disappear as Gilman suggested over seventy years ago, the home would survive their departure. In the last century and a half many changes have occurred in the physical house. Once fashionable styles are now quaint survivals, and technology has altered techniques of construction. But the symbolic house in America remains virtually unchanged. The aspirations and satisfactions experienced by the man who lived in one of A. J. Downing's Gothic cottages are the aspirations and satisfactions of today's suburban householder, whose house remains the evidence of property and the emblem of home." Why try to paraphrase a summary like this? It should be written out and prominently displayed in architecture schools all over the country. Maybe if more architects began

thinking in terms of what a house is about, what houses are meant to *do* in and for society, rather than simply about their composition and plumbing, there would be a lot fewer unemployed ones than there currently are. Better still, make the book assigned reading in architectural design as well as architectural history courses.

ALAN GOWANS  
*University of Victoria*

JOSHUA C. TAYLOR. *The Fine Arts in America*. (The Chicago History of American Civilization.) Chicago: University of Chicago Press. 1979. Pp. xvi, 264. \$17.50.

Joshua C. Taylor has written a useful, provocative short history of the arts—mainly painting—in America. The author is director of the National Collection of Fine Arts in Washington, a Smithsonian branch specializing in American painting and sculpture, which he has made into a fine scholars' museum. His book, as one would expect, is readable and literate. It will be read profitably by a wide audience, from beginning students to specialists, as it functions both as a quick survey of the field and as a thoughtful, iconoclastic summary of the current state of scholarship in the American field.

Beginning in 1670 and coming up to the present day, Taylor weaves a fluent discussion of major artists, patrons, institutions, cultural events, and stylistic developments without resorting to the traditional clichés, to "isms," or to art-historical jargon. American art is seen simply as one aspect of our history, and the author's clarity of thought and language properly signifies the extraordinary creativity of Americans over the years. At the very beginning, he rejects the popular question, "What is American in American art?" He comments that "for the nationally oriented mind,"—and, one might add, for most earlier historians in the field—"European influence becomes a subtle opponent, a force luring the artist away from his true national character." He thus rejects the traditional, now meaningless, groupings: Homer, Eakins, and Ryder as the great "native" American painters or Whistler, Sargent, and Mary Cassatt as the balancing triumverate of expatriates. Rather, Homer is here discussed in a short section entitled "Perception and Truth," which introduces Chevreul's theories and Corot's style before discussing the work of Eastman Johnson, then Homer, and finally bringing in Émile Zola! Eakins also is given a new place, without any diminution in his reputation; Taylor finds in his art "a taut astringency very nearly unique in America," and he concludes in a typical laconic, telling way: "Later critics have liked to cite Eakins's art as char-



acteristically American; few of Eakins's contemporaries would have agreed."

Taylor's initial chapter takes on the first century of our art, 1670 to 1776, and in a few pages makes sense of our earliest styles in painting, furniture, and architecture. Here as elsewhere the tiny marginal illustrations in black and white may serve as references for those already familiar with the objects, but I should think a beginner would have to use them in conjunction with better reproductions or the originals. This chapter gives promise of bringing to bear a knowledge of all the arts, but the subsequent ones develop an increasingly narrow focus on painting and sculpture. Architecture, furniture, silver, prints, even drawings and watercolors are given short shrift; presumably they fall outside Taylor's definition of "fine arts."

The second and third chapters take us from 1776 to 1900 and form the richest part of the essay—understandably, as this period is the author's specialty. The development of genre, landscape, and still life is nicely tied to changing American attitudes and to the major fairs of 1876 and 1893. History painting is given its rightful place, for a change, as is popular art (the prints of Currier and Ives, the plaster Rogers groups, and so on). This makes the total omission of "folk" or rural art all the more difficult to explain. Most American art in a sense represents a more or less naive reflection of high style art in England, France, or Germany; Copley relates to Reynolds as Ralph Earl or Winthrop Chandler relate to Copley. Failure to mention these "Connecticut school" followers of Copley, or to give any indication at all of our immensely rich, widespread folk tradition in the half-century before the Civil War, seems nearly inexplicable.

It is hard to argue with such omissions, however, as the author has promised us not a survey or a list but a suggestive essay; indeed both the book's strengths and its weaknesses lie in its idiosyncrasies. Thus, one can forgive inclusion of an eighteen-page chronology ("Events in Art," "Other Events"); though I cannot see its purpose, it may be useful to some. Less acceptable are a number of aggravating typographical and minor errors. It is even harder to justify having two illustrations of Catlin's work, one of George Loring Brown and James Hamilton (these four, like too many of the illustrations, are taken—apparently chauvinistically—from the National Collection of Fine Arts collection), while no mention at all is made of M. J. Heade, Fitz Hugh Lane, or Sanford Gifford. Taylor may believe that the term "luminism" has been misused or overused, but this is no excuse simply to avoid three of its major practitioners or to illustrate such an atypical, nonluminous example by a fourth, J. F. Kensett.

Two final chapters deal competently with the periods "1900–1945" and "Since 1945." Taylor dis-

cusses the concept of modernism, and, as before, gives emphasis to style, patronage, and popular taste. He avoids the old notion of art history as biography and in doing so tends to stress cultural pattern rather than individual genius. This is all to the good. And equally welcome is the author's constant stress on the variety of artistic inspiration in America: for example, the teens are seen both as a time of abstract experimentation and as one that saw the academicians retain their dominance. On the debit side, a continuing Washington bias must be noted; a greater problem is an increasing sense of fragmentation, as the author jumps from one artist to the next, as he tries in a short space to mention each of the major figures of our own incredibly busy century.

Finally, one might note that Taylor's bibliography is just two pages long and includes only one book on painting since Richardson's still nearly standard survey of 1956—that is, Barbara Novak's *American Painting of the Nineteenth Century* (1969). This is an intriguing citation, for no two recent books disagree more—both methodologically or critically—than Novak's and Taylor's: in fact, Taylor's current essay might almost have been written as an antidote to Novak's thesis, which selectively traces a mainstream American style that is by definition luminist, objective, and linear. Novak's book represents perhaps the last, best effort to isolate the qualities that make American art American; Taylor's may be the first to attempt a different, non-judgmental, historically-oriented synthesis. The two books, read together, provide as useful a survey of the fine arts in America as one can now obtain.

THEODORE E. STEBBINS, JR.  
Museum of Fine Arts,  
Boston

G. THOMAS COUSER. *American Autobiography: The Prophetic Mode*. Amherst: University of Massachusetts Press. 1979. Pp. vi, 222. \$15.00.

This book builds on a group of separable but related assumptions. (1) There is an important tradition of American autobiography, beginning with the colonial Puritans and Quakers, in which the self is conceived as representing the community at large. (2) Characteristically, the mode of representation is prophetic—the self embodies a vision of the community not as it is but as it ought to be. (3) This "prophetic mode" makes that very process of self-portrayal a means of denouncing what is by reference to the will of God. (4) The will of God, so defined, magnifies the autobiographical self to virtually cosmic proportions; the individual becomes at once the keeper of the higher spiritual laws and harbingers of American destiny.



None of these assumptions is original with G. Thomas Couser. As he acknowledges in his introduction, he culled them from various scholars in the field. What might be considered his own contribution is to trace the pattern through a series of personal writings spanning more than three centuries, from the Puritan "spiritual autobiographies" through the works of Woolman and Franklin to the predictable nineteenth- and twentieth-century figures: Thoreau, Whitman, Frederick Douglass, Henry Adams, Louis Sullivan, Frank Lloyd Wright, Gertrude Stein, Malcolm X, and Norman Mailer. The result is a sweeping and superficial overview of "American culture." The book reads like one of those all-too-familiar inter/multi/non-disciplinary courses in American Studies, a mishmash of personalities, ideas, and approaches, held together by a simplistic theme more or less chronologically developed.

To be sure, Couser tries hard to vary the theme. Following Daniel Shea, he distinguishes Quaker from Puritan concepts of the self. Following James Cox and William Spengemann, he shows how Franklin blended and secularized those concepts in making himself the exemplum of a rising nation. Following other critics and scholars (not all of them adequately acknowledged), he contrasts the uses of American selfhood in Thoreau, Douglass, and Sullivan and outlines the continuities from Whitman to Wright, Adams to Mailer. The "prophetic mode" lends itself to a variety of extremes: euphoria and despair, celebration and denunciation, self-vaunting and self-effacement. Couser notes the interplay between extremes, and describes how different autobiographers move toward one extreme or another. All this is worth reading as a summary of previous scholarship on the subject. But in its own right this book tells us virtually nothing new about "autobiography" as a genre or about "the prophetic mode" as a form of rhetoric and thought or, for that matter, about the anxiety of being American in any concrete historical or psychological sense. A common pattern is *there*, evidently, but that should provide the beginning for the inquiry, not its rationale and end.

Couser is at his best when the pattern he adopts leads him into controversial areas (for example, the ironic or ambiguous ending of Adams's *Education*). He is at his worst when he ventures into areas he apparently does not understand (Puritan rhetoric, for example). But by and large his tone has the blandness of extended synthesis and recapitulation. And therein may lie the book's chief value. Any work of summary has at least potential heuristic value; and by indirection, as it were, Couser may alert students of American autobiography to problems he himself fails to consider—for example, the social function of "the prophetic mode," the ide-

logical dimension of our "ongoing and almost obsessive inquiry into what it means to be an American" (p. 9), the formal and conceptual dynamics of a genre that thrives on "perpetual crisis" (p. 200), the tensions inherent in asserting oneself, verbally and figurally, on a "representative" scale, and most broadly, the psychic, imaginative, and cultural implications of transforming "American" from a civic into a "prophetic" identity, and of investing "America" (as the source and object of prophecy) with attributes of the divine.

SACVAN BERCOVITCH  
Columbia University

BARNET BASKERVILLE. *The People's Voice: The Orator in American Society*. Lexington: University Press of Kentucky. 1979. Pp. 259. \$14.50.

*The People's Voice* essays what amounts to a social history of American oratory. Discussing famous speeches and speakers from the 1760s to the present, Barnet Baskerville describes changing modes of rhetoric and changing attitudes toward oratory as reflections of the changing character of American society and politics. He is widely read in his subject—he has devoted his career to the analysis of American rhetoric—and his book touches gracefully and often perceptively on a host of familiar phenomena not often approached from his point of departure. In all these respects his volume makes a useful study, and it culminates in a deeply felt appeal to the present generation to restore some form of eloquence to public deliberations.

Nevertheless the book promises more than it delivers, and it falls far short of exhausting the possibilities of cultural or social history. (This is, perhaps, only to be expected of someone who is not a professional historian; but we must review the book as a contribution to historical inquiry.) On the most elementary level the history to which Baskerville relates oratorical developments is at best conventional, at worst superficial. In dealing with the period after the Civil War, for example, he invokes Morrison and Commager but makes no use of Kleppner and Jensen, whose work might better have underlain his analysis. More generally, his account often resembles an old-fashioned political history of the United States written with special reference to classic performances like Patrick Henry's orations, Daniel Webster's second reply to Hayne, and the Lincoln-Douglas debates but extended by reflections on such matters as Theodore Roosevelt's presidency and the Kennedy-Nixon campaign of 1960. (Interestingly, it mentions Woodrow Wilson only in passing.) The reader may indeed gain some new understanding of the rhetorical strategies em-

ployed on these occasions, but the rhetorical analysis and the historical analysis do not significantly enhance each other.

Neither can the reader develop any sure perception of the roles that oratory may have played in American society. Baskerville holds that the quality of public rhetoric coincided with the importance each generation attached to political events, but the proposition does not carry him far. On his own evidence, there were questions to be asked about how the evangelical tradition may have shaped American discourse, how certain fundamental characteristics of American society may have influenced its reception, and what literary criticism, social psychology, or cultural anthropology may tell us about the early republic's preoccupation with eloquence. Answers to questions like these are admittedly all too likely to be glib or formulaic, but it is difficult to see how else one can truly illuminate the role of orators and of oratory in American life.

RUSH WELTER  
Bennington College

W. ANDREW ACHENBAUM. *Old Age in the New Land: The American Experience since 1790*. Baltimore: Johns Hopkins University Press. 1978. Pp. xii, 237. \$13.95.

In recent years, following quickly on the heels of the previous decade's intense interest in the history of family life, there has emerged a small but growing body of work on past attitudes to the various stages of life. First it was childhood, then death; more recently, adolescence and old age. Slowly, perhaps warily, we are closing in on the middle. In each case initial work has been bold and speculative—followed by deeper, more cautious, more detailed analyses. As for the study of old age in America, the former approach was best represented by David Hackett Fischer's *Growing Old in America* (1977); for the latter we now have W. Andrew Achenbaum's *Old Age in the New Land*. Mention of Fischer's seminal work, at least in passing, is imperative here—if unfortunately so—since Achenbaum does not discuss it at any length and even fails to include it in his bibliography (while dutifully listing James Fenimore Cooper's *The Prairie* and the stories of Sarah Orne Jewett), despite the fact that Fischer's book is the only other full-scale study of the subject and that Achenbaum read that work in manuscript and reviewed it (quite critically) well before his own book was published.

Now, having said that, let me quickly go on: *Old Age in the New Land* is an insightful, carefully constructed, well-argued, complex, and important book. Without falling into the common trap of exalting the past as a golden age of invariable domestic warmth and intergenerational cooperation, sup-

port, and respect (from which we, of course, have sadly fallen), Achenbaum does carefully trace the steady degradation of the aged in the post-Civil War years—with, he suggests, an abatement of the worst of those conditions during the past several decades. Eschewing the temptation to interpret this general decline in status as directly consonant with the familiar shift, in so-called high culture, from romanticism to realism (indeed, one of the particularly insightful aspects of Achenbaum's entire study is its careful delineation of the differences between the nature and chronology of intellectual and social change relating to the aged during the course of American history), he weaves an intricate web of explanation involving the overlapping of scientific, bureaucratic, and popular thought with the emergence of major changes in medical and economic realities.

This is an ambitious book, doubly so because it attempts to cover so much in so little space: 190 years of history in 171 pages of actual text. Thus, while it is remarkably dense and complex for a work its size, it is inevitable that this limitation be evident somewhere. That somewhere can be seen most clearly in the heavy burden a relatively frail number of sources must carry; this is especially true when the author is attempting to elucidate the attitudinal aspects of his argument—that is, when he is working the side of the street generally given over to the cultural and intellectual historian. Much of the argument late in the book concerning relative states of well-being among the aged at different times in recent history, for instance, relies disproportionately on government economic data. Although he does not explicitly engage recent controversies concerning modernization theories in his historical narrative, Achenbaum does in the end construct an argument that is in large measure consistent with such theory.

*Old Age in the New Land* is, finally, a work of sophistication and compassion—a work that, despite its disagreements, is complementary to Fischer's pioneering study and a work that should stand for some time as a landmark in the analysis of the historical meanings of old age in America.

DAVID E. STANNARD  
University of Hawaii

RICHARD L. BUSHMAN *et al.*, editors. *Uprooted Americans: Essays to Honor Oscar Handlin*. Boston: Little, Brown. 1979. Pp. xvii, 366. \$15.00.

Few historians of recent decades have established a larger presence within the discipline of history than has Oscar Handlin. Scholar, teacher, academic entrepreneur, critic of the discipline and the profession—Handlin has expended his restless energies in

a hundred distinctive and consequential ways. Students, both graduate and undergraduate, scholars enjoying the amenities of the Charles Warren Center, investigators of American social history, colleagues who have learned from and argued with him about the study of the past—the list of persons who have been influenced by him is large. How appropriate, then, that some of his former students should gather together a group of their own essays to honor him.

If a teacher's stature is measured in some part by the students who claim his or her tutelage, then the roll call of contributors to this volume offers impressive evidence of Handlin's standing. Not many of us can claim such distinguished lineage. Nor are there many whose students have ranged so widely or so effectively through American social and cultural history. Certainly it is true that the diversity of topics included in this volume reflects primarily the developing interests of the authors themselves. And yet, as Richard L. Bushman points out in his introductory statement, Handlin's own eclecticism—he posed “no formal method beyond the forceful and intelligent use of fact and reason”; he transmitted “no sense of forbidden or preferred paths” (p. x)—encouraged his students to strike out on their own. Even Handlin's increasingly vociferous “aversion to formalized hypothesizing” and “overwrought social-scientific methodology” (p. x) has not stood in the way of his students' engagement with the social sciences. Richard L. Bushman and John Demos give evidence of that.

The organizing theme of Handlin's scholarship, Bushman reminds us, has been the “systematic study of plain people” (p. x). In one way or another, each of the ten essays presented here bears witness to that concern. Every reader, depending upon taste and inclination, will find his or her favorites among them. My own include Demos's marvelously evocative account of witchcraft in the historical experience and memory of a New Hampshire community; Bushman's sensitive discussion of the revolutionary generation's rejection of dependent patronage assumptions in political and social relationships; and Neil Harris's imaginative analysis of the “consciousness revolutions” reflected in American utopian fiction of the late nineteenth and early twentieth centuries. But there are others—by Anne Firor Scott, Roland Berthoff, William McGloughlin, Paul Goodman, William Taylor, Arthur Mann, and Moses Rischin—that merit attention as literate and informed essays in historical exposition. Barbara Solomon offers a brief word portrait of Handlin at the beginning, and Robert Mirak provides a selected bibliography of Handlin's published works at the end.

Oscar Handlin has in recent years lamented his sense of the collapse of scholarly community among

historians. He reports that he has felt himself standing alone, reading and writing each book, he has said, as if it followed and preceded nothing else, as if the scholarly enterprise now lacked both context and connection. Surely these essays offer evidence that important connections still exist and that scholarly community, though dramatically more diverse than it used to be, remains a reality.

JOHN HOWE  
University of Minnesota,  
Twin Cities

W. STITT ROBINSON. *The Southern Colonial Frontier, 1607–1763*. (Histories of the American Frontier.) Albuquerque: University of New Mexico Press. 1979. Pp. xvii, 293. Cloth \$12.50, paper \$6.50.

When Frederick Jackson Turner in 1893 told us about the significance of the American frontier he made such an impact upon American historical writing that the reverberations echoed far into the next century and continue even today. W. Stitt Robinson's narration of the expansion of the southern frontier in the era, 1607–1763 is the latest addition to the projected fifteen-volume series of the American frontier edited by Ray A. Billington and Howard Lamar that is designed to round out the frontier experience as originally suggested by Turner. Some of these volumes, published and scheduled for print, are topical dealing with the fur trade and law on the frontier; others like Robinson's volume are chronological segments of frontier history tracing a steady advance from earliest times through the end of the nineteenth century. Very probably the volumes of chronological narratives will end about 1893, the year that Turner saw by census data that the frontier had passed. Since much of Turner's analysis had come from colonial and early frontier developments east of the Mississippi, it is appropriate that Robinson's study fill in gaps left by Douglas Leach's *Northern Colonial Frontier*, Jack M. Sosin's *The Revolutionary Frontier*, and Reginald Horsman's *Frontier in the Formative Years, 1783–1815*. These and other volumes in the series generally set forth themes of progress under Turner's theoretical scaffolding of ideas placing emphasis upon an “Americanization” process; a frontier influence upon the formation of the American character and the growth of political rivalries; a traditionalist environmentalism (emphasizing the impact of the wilderness upon pioneers rather than their impact upon the land and Indian people); and the emergence of sections and sectional conflict. Robinson, for example, writes about a southern frontier as opposed to northern and middle colony frontiers and recognizes the beginnings of sectional rivalry between the tidewater settlers and the inte-

rior frontier society. And, like Turner, he argues against a "monocausationist" approach to frontier history and yet at the same time says that the frontier was "one of the strongest factors" in shaping the growth of a predominately agricultural society. Robinson's book follows Turner's lead in telling a story of Anglo-European settlers' expansion into the wilderness. There is a lot about Indians in this book but not much about Indian people in what is now called ethnohistory. Nor are blacks recognized as having any real force in early frontier history.

Robinson, a seasoned scholar, gives us a thoroughly documented series of chapters based upon wide investigation of both sources and secondary works. He begins his book with the first settlement of Virginia and early experiments in agriculture and manufacturing at Jamestown. His descriptions of Indian attacks are given to us as "concerted action" (rather than as conspiracies). Maryland's manorial society, prospering on a tobacco economy, is closely linked to Virginia. Bacon's rebellion is explained in its multiple causes, and Bacon himself is rejected as "a torchbearer of the Revolution." Thereafter follows a narrative of the emergence of a Carolina coastal frontier and a fur and skin trade penetrating hundreds of miles into the southern wilderness.

Although the Indian slave trade is mentioned, we might have had more of an analysis of its complexity and its relation to black slavery and Indian politics coming out of frontier violence and the Yamasee War. Nevertheless, Robinson is to be congratulated for weaving a large mass of detail into a readable narrative based upon the origin and growth of the southern colonies and their common frontier up to the time of "the War for Empire." And throughout the book there is the essential thread of frontier expansionism. Here, then, is a volume in the tradition of Turner and another predecessor many of us admire, Verner Crane.

WILBUR R. JACOBS  
University of California,  
Santa Barbara

DAVID THOMAS KONIG. *Law and Society in Puritan Massachusetts: Essex County, 1629-1692*. (Studies in Legal History.) Chapel Hill: University of North Carolina Press. 1979. Pp. xxi, 215. \$21.00.

David Thomas Konig has introduced a significant dimension to the debate on Puritan Massachusetts' transition from a communal to a more modern, impersonal society in this closely reasoned study of the relation between the development of legal institutions and social change in Essex County. It is his contention that the colony's relative stability in the

midst of profound change was due largely to the magistracy's ability to reconcile conflicts and maintain social control. Unlike most recent historians, he regards the remarkable litigiousness of the settlers after 1660 as a method of integrating the society rather than as a symptom of "declension." Given the floundering state of Puritan studies, this is a most helpful hypothesis.

The author also asserts, apparently as a corollary, that the towns and churches played relatively insignificant roles in forging and maintaining order. This provocative suggestion is more dubious. If correct, it would force a substantial reinterpretation of American Puritanism. Unfortunately, Konig's sketchy portrait of town and church government is static. He dismisses too readily the possibility that these institutions developed in a manner roughly parallel to the court system. The Scotch verdict of "not proven" seems just on this portion of his argument.

Yet however exaggerated his notion of the rigidity of all local institutions except the court, Konig has demonstrated that a lively picture of socioeconomic change can be gleaned from the court record. No scholar has read those voluminous manuscript records more sensitively. His skillful melding of social and legal history is of great value and makes fascinating reading. Of particular interest, especially to nonspecialists, is his account of the struggle between those who favored and those who opposed a legalistic approach to order and the impact of that struggle on the witchcraft outbreak and subsequent trials in 1692. Historians have long held that the timing of the crisis was related to the great uncertainties of the Dominion era. Yet few have been able to find a credible link between high politics and local hysteria. Konig has found one in the conflict over the authority of local legal institutions whose legitimacy had been compromised by the revolutionary circumstances of the period.

Recent colonial historians have been battering at the dominant town-centered approach to New England. This book, rich in ideas and astute in the use of evidence too often ignored, contributes markedly to the trend. By shifting the focus to the courts and by substituting concepts from anthropology for ones derived from demography, it offers an alternative method as well as criticism. The book's merits are considerable and its arguments ought to be pondered.

RICHARD P. GILDRIE  
Austin Peay State University

BRUCE C. DANIELS. *The Connecticut Town: Growth and Development, 1635-1790*. Middletown, Conn.: Wesleyan



University Press; distributed by Columbia University Press, Irvington, N.Y. 1979. Pp. xii, 249. \$16.00.

Bruce C. Daniels welcomes the findings yielded by the case-study approach to an analysis of New England's early towns. He believes, however, that these findings—series of microscopic slides, as he terms them—must be merged, along with other materials, so as to produce colony-wide or regional pictures. In requiring a wider angle of vision, he follows the lead of Michael Zuckerman's *Peaceable Kingdoms* (1970) and Edward M. Cook's *Fathers of the Towns* (1976). In seeking useful models among the social sciences, he conforms to the current colonialist norm. Treating the evolutionary aspects of town settlement and formation, of population and its distribution, of town-meeting government, of ecclesiastical societies and other proliferating local institutions, and of central places, he argues that institutional elaboration and economic interconnection led Connecticut's towns to the threshold of urbanization.

Daniels's study—easily the most ambitious examination of Connecticut communities to appear to date—is a most useful account. Yet it is marred by numerous flaws. At times data are mishandled. A failure to take account of shifting county boundaries, for example, produces important errors in the detailing of population growth. Again, the misreading of shipping statistics underlies a totally inaccurate account of the distribution by town of Connecticut-registered vessels. Borrowing from the social sciences is done with unequal success. Daniels skillfully employs geographical techniques, integrating them with his data and providing a series of first-rate maps. In other cases, however, the meshing of evidence and contributing discipline is awkward and incomplete, models being jarringly appended to what is, essentially, traditional institutional history. The study suffers from a further lack of integration in its concentration on individual structures of town life without adequately relating them to the whole. To pick a superficial illustration, the decline in the number of town meetings, especially noticeable between 1721 and 1765 and here accorded major importance, obviously is related to the appearance of proprietors' and society meetings, their business having formerly been transacted in the town forum. Yet no connection is drawn. Finally, Daniels is handicapped by the uneven quality and paucity of specialized studies relating to Connecticut's towns. Reliance on this work causes him to emphasize a nonexistent shift from an economy geared primarily to agricultural production for local consumption to one heavily oriented to export markets. And the literature's lacunae account for a picture of the maldistribution of wealth in urban centers that depends far more on Henretta's and

Kulikoff's studies of Boston than on any close look at the towns in question. Such problems suggest that Daniels's attempt to synthesize is premature.

BRUCE E. STEINER  
Ohio University

EDWIN S. GAUSTAD. *George Berkeley in America*. New Haven: Yale University Press. 1979. Pp. xi, 225. \$15.00.

That George Berkeley, one of the most brilliant of British philosophers and ultimately a bishop in the Anglican church, a friend of Addison, Swift, Pope, and Steele, should have spent nearly three years of his mature life in Rhode Island is perhaps the most amazing improbability of our cultural history of the early eighteenth century. The rudimentary facts of Berkeley's mission here—and only a religious or moral mission could have brought him—have long been known, but with the publication of this useful book by Edwin S. Gaustad, who is an authority on colonial religion, we have the whole odd story for the first time pulled together in one place and clearly explained.

Berkeley arrived in Rhode Island in January 1729 with the whiggish intention of rescuing Britain and the empire from corruption and decay by establishing in the New World a seminary of religion and virtue, specifically an Anglican college from which would pour forth men of good morals and learning. He also was possessed of the dream already a century or two old of using such an institution to train Indian converts who would themselves become missionaries to their kindred. The philosopher's original site for this fountain of world redemption was Bermuda, but, as Gaustad shows, in the face of criticism and even ridicule, Berkeley had probably already decided on a mainland location before he sailed from England.

The execution of the plan was contingent on the allocation of £20,000 from Parliament, which Berkeley, not unrealistically, felt assured of getting. He already had a royal charter. The whole scheme collapsed, however, when Parliament failed to deliver on its promise, one cause being objections raised in Virginia, where William Byrd II, James Blair, and the royal governor, William Gooch, all wrote home opposing the scheme. Ironically, they learned of Berkeley's plans when the ship that brought him over to Rhode Island was forced to land first, after four months at sea, in Virginia in order to reprovision. Berkeley then spent ten days in Williamsburg. It remains a mystery why he had not years earlier recognized the College of William and Mary as an already existing Anglican foundation in America and instead tied his hopes to the Bermuda



location. He may have felt that only an entirely new institution, developed under his direct guidance, would suffice.

Yet despite the total failure of the college idea, the colonies benefited incalculably from the presence of the Irish universal genius. The stimulus Berkeley gave to American philosophy through Samuel Johnson, who later became the first president of King's College (Columbia), is well known. But Berkeley also endowed the first graduate fellowships in America (at Yale), gave unparalleled gifts of books to both Harvard and Yale, purchased a pipe organ for his church in Newport (one of the first of these instruments in the English colonies), and brought with him in his coterie of future "faculty" the painter John Smibert, usually considered the best portraitist to come to America in the eighteenth century (Smibert settled permanently in Boston and influenced Copley and others). In sum, everywhere Berkeley turned he was enriching, not least when he preached regularly on Sundays and functioned diversely as a churchman. As dean of Derry, Berkeley was the highest ranking Anglican to set foot in the American colonies, and Gaustad rightly emphasizes that those services to America that Berkeley himself most valued were spiritual.

NORMAN FIERING

*Institute of Early American History and Culture*

JEROME H. WOOD, JR. *Conestoga Crossroads: Lancaster, Pennsylvania, 1730-1790*. Harrisburg: Pennsylvania Historical and Museum Commission. 1979. Pp. xi, 305. \$8.50.

*Conestoga Crossroads* recounts social, political, and economic developments in Lancaster, Pennsylvania, between 1730 and 1790. In this pleasantly written and handsome volume, Jerome H. Wood, Jr., offers a comprehensive survey of life in this largest inland town in British North America. Following a brief description of the town's origins, the volume divides into three topical parts covering the years 1742 to 1790. The first part portrays the town's political history; the second, economic structure and the vital role of inland commerce; the third, society and culture, with special emphasis on religious life. Wood's book is traditional history in the best sense of that maligned term; he has culled the documentary record, and his account is spiced by apt quotations; yet the author also includes some of the newer counting techniques in his examination of social and economic structure.

Colonial historians will find Wood's volume useful as an urban complement to James T. Lemon's regional history of southeastern Pennsylvania, *The Best Poor Man's Country* (1972). Wood necessarily pursues many of the same themes developed by

Lemon—multi-ethnicity and cultural pluralism, privatism in the economic sphere, accelerating social and economic stratification, denominationalism in religion, and limited political involvement despite the existence of democratic institutions. Wood has the advantage over Lemon on the matter of scale as his study of a single town permits him to probe more deeply. The inland trade, for instance, is carefully described, and his account of this engine of growth is nicely punctuated by abundant quotations drawn from Lancaster and Philadelphia merchants. The reader might have been given a bit more quantitative evidence on the trade areas carved out by Lancaster, particularly since the author implies that Lancaster eventually lost its grasp on the inland wholesaling trade. Indeed, some of the author's own tables suggest that the town's growth slowed somewhat in the quarter century before the Revolution, but these data are inadequately tied to shifting trade patterns. Equally revealing are the chapters on ethnicity and religious life. Clearly this was a diverse town, with a majority of Germans and an abundance of English, and these nationalities were in turn divided among a variety of religious denominations. Wood's engaging discussion of the Great Awakening in Lancaster exposes the tense doctrinal differences that served to weaken community and, at the same time, to strengthen privatism.

One comes away from this volume having learned a great deal but with the worry that somehow the expertly crafted essays on Lancaster's politics, economy, and society fit together uneasily. Lancaster has been described as a volatile mixture—of privatism, denominationalism, cultural pluralism, political apathy, and social stratification—and we must wonder at the remarkable and largely unknown forces of order that kept the town from exploding. Had Wood taken us this last step, his volume would qualify in my judgment as a great book instead of the very good one that it is.

CARVILLE EARLE

*University of Maryland,  
Baltimore County*

W. ROBERT HIGGINS, editor. *The Revolutionary War in the South: Power, Conflict, and Leadership. Essays in Honor of John Richard Alden*. Durham, N.C.: Duke University Press. 1979. Pp. xxii, 291. \$17.75.

Between 1956 and 1971 the noted military historian John Richard Alden guided nearly a score of students through the doctoral program at Duke University. *The Revolutionary War in the South* is a tribute by eleven of those students to Alden's skill and dedication as a teacher and as a scholar.

In form and subject matter, this book is an ap-

propriate tribute. Following the editor's brief preface explaining the genesis of the work, W. W. Abbot skillfully surveys and evaluates the corpus of Alden's scholarship. The collection begins with four essays on "The Context of the Revolution in the South"; by Jack P. Greene, who offers a study of three leaders of Revolutionary Virginia, Richard Henry Lee, Edmund Pendleton, and George Mason; by W. Robert Higgins, who discusses the impact of the Revolution on the South's slaves; by James H. O'Donnell, on the impact of the Revolution on Southern Indians; and by R. Don Higginbotham, on James Iredell and North Carolina politics. The second section of the book deals with "The Role of the Individual" and consists of essays by John C. Cavanagh on Benjamin Lincoln, and Paul David Nelson on Horatio Gates, both focusing on their Southern campaigns, and pieces by Louis W. Potts on Arthur Lee as a diplomat, and Richard J. Hargrove on John Laurens. The collection closes with three pieces on "The Framework of Military Conflict in the South" by Ira D. Gruber on Britain's Southern strategy; Clyde R. Ferguson on the partisan-militia in South Carolina; and Eldon Jones on the withdrawal of British forces from the South at the end of the war.

If this recitation of the collection's contents seems rather flat, it should at least give the reader an accurate foretaste of the intellectual stimulation to be derived from most of these essays. Only two of the eleven pieces are of real quality. Jack Greene's portrait of three Virginia leaders, although prefaced by an unnecessarily complicated analytical apparatus, is a sensitive and penetrating study. And it makes at least one arresting point: George Mason was uniquely effective as a constitution-maker in Revolutionary Virginia in part because he truly manifested the patrician disdain for political life that other gentry leaders thought they ought to feel. Thus when Mason came out of retirement in response to a real political crisis, his countrymen were deeply impressed by his sacrifice. The second important piece is Clyde Ferguson's creative integration of old and new interpretations of the military history of the Revolution as espoused, for example, by John Richard Alden and John Shy. Ferguson argues persuasively that South Carolina's partisan guerrilla forces were often militia, and a more seasoned militia than Shy and others have realized.

Several other essays merit brief comment. Higginbotham's reinterpretation of Revolutionary politics in North Carolina is sensible and perhaps important but not especially exciting. Pott's probing study of Arthur Lee, while probably less important, is interesting and well written. Hargrove's more speculative—and factually sloppier—psychobiographical portrait of John Laurens is less per-

suasive; Nelson's strained apology for Horatio Gates is less stimulating.

The remaining essays deserve little attention. Higgins shows refreshing candor, in a fairly traditional Southern historian of the South, by admitting that the Revolution did nothing for the South's slaves but he has nothing new to say. Cavanagh's defense of the ineffectual Benjamin Lincoln is muddled and tedious; Gruber's insipid analysis of Britain's Southern strategy is, coming from a prominent military historian, a keen disappointment. Finally, O'Donnell's bare summary of the wars between Southern whites and Indians and Jones's recitation of the logistics of the British withdrawal are simply pointless.

RICHARD ALAN RYERSON  
*Papers of William Penn,*  
*Historical Society of Pennsylvania*

JACK N. RAKOVE. *The Beginnings of National Politics: An Interpretive History of the Continental Congress.* New York: Alfred A. Knopf. 1979. Pp. xvii, 484. \$15.95.

This volume might best be described as an oddly conceived book, reflecting the author's latitudinarian attempt to accomplish too much without proper grounding in the vast array of primary source materials available to students of the Continental Congress and the Revolutionary era. Then, again, the problem may be that Jack N. Rakove could not make up his mind about what was the subject of his book. As a result, readers will inevitably become confused. Is this a history of the Continental Congress? Or the evolving doctrine of American federalism? Or the individuals (both strong and weak, important and secondary) who struggled to direct the war effort and the complex process of nation-making? The author undoubtedly would answer in the affirmative on all counts. Unfortunately, the integration of topics is so haphazard that only knowledgeable specialists will be able to make much sense or glean new findings from what may be characterized as a discursive, if not impenetrable, volume.

It would be appropriate to reflect on each of Rakove's concerns. In his history of the Congress, the author has sought to overcome what he repeatedly labels as the "simplistic" interpretations of Merrill Jensen, H. James Henderson, and E. James Ferguson. Rakove, it seems, has discovered that factions were not very paramount to explaining the decision making of Congress, at least until 1779. Terms like "radical" and "conservative" (except when Rakove employs them) are misleading, as are notions of voting blocs in Congress. It thus appears that men as different in temperament and thinking as Samuel Adams and John Dickinson were really just seeking

solutions to an unending list of vexing problems. In fact, it is possible to speculate that Rakove got himself trapped into analogizing from a modern corporate model of decision making: individuals taking up issues, debating the policy alternatives, accepting group consensus, and burying any record of formative disagreement for the good of the corporate image. This type of explanatory model simply is out of place in the turbulent Revolutionary political arena, where personal, local, regional, and sectional differences made decision making, especially in Congress, a nightmarish if not paralyzing task.

But Rakove refuses to admit that the paralysis of Congress, surely setting in by 1777, might have affected the formulation of policy (or its relative absence) and, even more important, questions about the distribution of powers between the states and central government. It is in this context that the author asks: Why did so many men avoid extensive service in Congress? His conclusion: service was not a matter of seeking power but of reluctantly accepting the burdens of republican self-sacrifice. (Another plausible answer might have been that patriot leaders sought power where it was, which was not in Congress.) Rakove's assertion of republican self-sacrifice on the part of the delegates is curious, moreover, since he claims that Revolutionary ideology had little impact on their actions or on the postwar debate over the concept of federalism (pressing issues of the moment were far more formative). Yet, why did ideology only affect behavior but not thought? If he had admitted both, then Rakove would have had a real dilemma, since he refuses to admit that very real differences over the fundamental issue of national authority—both in and out of Congress—reflected deep-seated ideological differences between localist and nationalist leaders.

A further problem has to do with Rakove's observations on the emergence of the doctrine of federalism. So much of the thrust in nationalist thinking arose from powerful minds in Washington's officer corps. Rakove, however, fails to explore that dimension, presumably because it was beyond Congress. Indeed, except for the nationalist attempt to infuse Congress with greater authority in the 1781–83 period, the author's presentation will do little to enlighten historians about the background of the nationalist upsurge that resulted in the Constitution of 1787. Rakove may have even further confused the issue by placing so much emphasis on James Madison.

This book does have some strong points. The discussion of the various drafts of the Articles of Confederation is fresh, even if it does not affect the interpretive substance of what already is well-known. His characterization of irascible Thomas Burke of North Carolina is revealing, adding to knowledge about that much-neglected personality. The role of

the Deane-Lee controversy represents the strongest material in the book, even if the author misses its full implications (such as its impact upon the creation of the nationalist bloc before 1781). On the other hand, the portrait of Robert Morris suggests selective sampling of the financier's correspondence. Few historians will be satisfied with a portrait of Morris as a man being tossed to-and-fro by others.

In its various aspects, this book leaves a great deal to be desired, except for those who have already decided that the interplay of factional alignments and political ideology had little impact on the experience of the Revolution—and its outcomes. At best, Rakove's work will supplement important studies by Jensen, Henderson, Ferguson, and Jackson Turner Main (on state politics). Despite dust jacket hyperbole, Rakove has not offered a fresh alternative for interpreting the Congress and the American Revolution.

JAMES KIRBY MARTIN  
University of Houston

STEVEN R. BOYD. *The Politics of Opposition: Antifederalists and the Acceptance of the Constitution*. (KTO Studies in American History.) Millwood, N.Y.: KTO Press. 1979. Pp. xiii, 178. \$18.95.

This slender volume contains a large body of information about the politics of the ratification of the Constitution. From the discussion of the document in Congress to the state legislative debates, the election of convention delegates, and the first national elections, the process is meticulously catalogued. Few errors mar the narration, several errors or misconceptions of previous chroniclers are corrected, and many bits of fresh data are supplied. The author does occasionally lapse into ill-considered generalizations. "Maryland, like the other middle states, was solid in its support for the Constitution" (p. 32); every state had a history of "bi-partisan" politics (p. 41); Stockbridge and Great Barrington, Massachusetts, were "urban centers" (p. 61); the efforts of Pennsylvania Antifederalists "led to" the Antifederalist victory in North Carolina and the "virtual draw" in Virginia (p. 95). Despite such slips, this is as concise and accurate a summary account as one is likely to encounter.

Unfortunately, Steven R. Boyd has also chosen to argue a thesis, and in that effort he is less successful. Since every attempt to amend the Articles of Confederation had failed, and Antifederalists constituted a majority in 1787–88, he asks, how does one explain the Federalists' triumph and the Antifederalists' acquiescence in their defeat? The usual explanation is that the Federalists were well organized and the Antifederalists were not. To the contrary, Boyd insists, the antis began organizing early

and had a vigorous campaign in operation throughout the fall of 1787 and the winter and spring of 1788. Their failure, rather, and the ultimate acceptance of the Constitution by all parties, derived from a tactical error: by agreeing to do battle on the issue "within the framework of the proposed Constitution," the Antifederalists unwittingly functioned as a loyal opposition and thus "contributed to the ultimate legitimization of the very Constitution they opposed" (p. 67).

The thesis is flawed in several respects. The antis were actually well organized only in Pennsylvania and New York, and they never managed to agree either on tactics or on basic aims. The absence of such agreement is the absence of "organization." More importantly, Boyd's question rests upon an unsound premise. Why did the Constitution meet with a different reception from that of proposals to amend the Articles? The answer is that it did not. Only one or two states, in each instance, had voted to reject the earlier amendments, and only two states voted to reject the Constitution. The difference was that the rules were changed: unanimous approval was required to amend the Articles, and only nine states were required to approve the Constitution.

In the end, then, the question is why did the Antifederalists go along with the changing of the rules? The alternatives Boyd suggests—forcible resistance or refusal to participate—were not options at all. The fact is that the antis were outwitted: superior intelligence, determination, and organization won out over numbers. For once, the good guys triumphed. As I have suggested elsewhere, that was in the order of the miraculous.

FORREST McDONALD  
University of Alabama

WHITMAN H. RIDGWAY. *Community Leadership in Maryland, 1790–1840: A Comparative Analysis of Power in Society*. Chapel Hill: University of North Carolina Press. 1979. Pp. xxi, 414. \$19.50.

The expansion of the suffrage and the introduction of the spoils system have led most historians to conclude that the American political process was more democratic by the 1840s than it had been fifty years earlier. Recently, however, as part of his attack on the "egalitarian myth," Edward Pessen has disputed the real importance of these changes for the way that power was wielded in antebellum society. The distribution of wealth was becoming more lopsided and so seemingly was the distribution of power. In *Community Leadership in Maryland*, Whitman H. Ridgway presents a modified version of the traditional view and concludes, in direct contrast to Pessen that, "the structure of authority in Mary-

land's political institutions conformed to Tocqueville's general description of democratic government. The ultimate power behind government rested with the people, while government responsibility was divided through various divisions and balances" (p. 189).

Arguing that crucial questions about the "evolution of American democracy" must be examined at the community level, Ridgway focuses upon the local context of four economically diverse counties: Baltimore City; rural, but socially heterogeneous, Frederick County in the central part of the state; slave-holding and tobacco-growing St. Mary's County south of Baltimore; and Talbot County on the Eastern Shore, primarily a grain-producing area. Ridgway analyzes changes in the nature of the decisional elite in each community between the first and second party eras and, by comparing the patterns of change in the different types of counties, asserts his basic thesis that growing economic and social diversity undermined oligarchic control of local communities during these years. While agreeing with the traditional view, he pictures the process as more gradual and complicated than have previous historians. Wealthy and prominent men continued to dominate the power structure well into the second party era, and change proceeded at different rates in different areas as a "function of a community's underlying social and economic heterogeneity."

Ridgway has struggled to specify his concepts and relate his study to relevant work in the social sciences. He challenges Pessen's use of "democracy," "egalitarianism," and "elitism" and attempts to define each more precisely. Rather than simply examining the social position of office holders, Ridgway distinguishes between the decisional elite (those participating in and significantly influencing key community decisions) and the commercial, propertied, and positional elites ("men with a certain resource"), and he carefully analyses the ways these groups overlapped. In this, as in his emphasis on the relationship between pluralism and democracy in America, Ridgway follows the general position set out by Robert Dahl in *Who Governs?*

Although Ridgway makes a plausible case for his main thesis, it seems unlikely that reading this book will convince those who have continued to resist Dahl's original formulation of the pluralist model. The author compounds this skepticism by failing to operationalize key concepts once he defines them and by introducing in his conclusion a number of slippery new concepts with little attempt at clear definition. Yet, given these problems, Ridgway has written a readable and quietly intelligent book that presents the most reasonable discussion available of the democratization of community leadership during these years. It is thoroughly researched and de-



serves to be read by every serious student of American political development.

WILLIAM G. SHADE  
Lehigh University

JOHN L. HAMMOND. *The Politics of Benevolence: Revival Religion and American Voting Behavior*. (Modern Sociology Series.) Norwood, N.J.: Ablex Publishing. 1979. Pp. xi, 243. \$16.50.

Increased understanding of the influence of religious values on politics has benefited political history. This book, whose useful points might have found adequate exposure in an article or two, shows that a good thing can be carried too far. John L. Hammond argues that the revivals of the 1820s and 1830s initiated by Charles G. Finney created a "revivalist political ethos" in New York and Ohio leading certain communities to support abolition, antislavery, temperance, and other moral causes. This notion is not new, but Hammond pushes it to extremes both in making sweeping claims about its impact before the Civil War and in extending his thesis into the twentieth century down to the 1960s. The writing makes the book difficult reading, but more importantly Hammond did not do the primary research needed to support his characterization of the revivals.

Hammond's key chapter, "The Revival Movement," relies on secondary sources and Finney's published works. The author simply assumes that the content of thousands of sermons preached for over a decade in hundreds of churches throughout New York and Ohio closely resembled Finney's preaching. He also assumes that revivalism led communities uniformly toward moral reform. These assumptions are not warranted either by the standard historical literature or by recent inquiries into revivals by younger scholars such as Paul Johnson and Linda Pritchard. Further, researchers at the Center for the Study of Early Religious Life at Ithaca College have found that revivals functioned often to integrate communities rather than raise divisive issues.

Hammond measured revivalism from 1825 to 1835 primarily by counting the revivals in each county reported in ten religious periodicals. His methods made for an encounter with only the tip of an iceberg. His honest admission, in an appendix, that "the most important variable could be measured with only moderate precision" (p. 216) is an understatement. Now moderate precision in a historian is no sin, but Hammond did not conduct sufficient research in local sources to get some sense of the enormously diverse and complex religious life of the period.

Hammond emphasizes most the causal link be-

tween revivalism and "political abolitionism," asserting that "those who experienced revivals also voted abolitionist" (p. 69). To make this claim, Hammond must bury a central fact that has emerged from recent writings on nineteenth-century politics and religion, namely that very few of the great mass who participated in revivals went on to become abolitionist or vote for the Liberty Party.

The backbone of the book is statistical analysis of revival measures with other county-level political and demographic data, not only for the years from 1830 to 1850 but also for 1856, 1860, 1870, and beyond. One might have thought 1850 the outer limit for taking data of uncertain reliability from 1825 to 1835 and stuffing it into a multiple regression formula. But Hammond presents tables combining data from 1825-35 down to 1940, and even generalizes about "revivalist areas" in New York in the 1960s. The further in time he goes, of course, the less his data have to do with the populations discussed. One does not need to be a statistician to realize that at some point the limits of absurdity are reached and crossed. The analysis of county-level data can be moderately useful, but should have constituted the *beginning* of description rooted in historical context, not, as in this case, the *end* of description, and much less of explanation.

RONALD P. FORMISANO  
Clark University

RUSSELL E. MILLER. *The Larger Hope: The First Century of the Universalist Church in America, 1770-1870*. Boston: Unitarian Universalist Association. 1979. Pp. xxviii, 1,009. \$25.00.

*The Larger Hope*, by Russell E. Miller of Tufts University, is a massive, largely institutional study of the first century of the Universalist Church in America. The author, not himself a member of the denomination about which he writes (it merged with Unitarianism in 1961), has pored over voluminous records, minutes, periodical files, biographies, and other primary and secondary literature dealing with Universalism. The work is organized topically in 8 sections and 29 chapters, with 99 pages of backnotes, 49 illustrations, a selected bibliography, and an index. Miller has told much of his story through attention to lay and clerical leaders who rose to prominence; their names stud these pages.

The Universalist Church—not so named formally until 1870—traced its origins to the arrival of Englishman John Murray (1741-1815) in America in 1770. Its first church was organized nine years later, and what became the Universalist General Convention was formed in 1793. Thereafter, the denomination grew swiftly; though reliable statistics are lacking, it was probably fifth or sixth largest in



the country in the 1830s and was at the peak of its national influence (though declining relatively in numerical rank) in the 1840s and 1850s. Universalism appealed to many Americans fleeing from strict Calvinism, for its theological basis stressed the love of God and the salvation of all (it was turned in a unitarian direction by its most prominent theologian, Hosea Ballou [1771–1852]). The church also suited the needs of a culture in which individualistic and localist trends were strong: “Universalism as a separate denomination developed in a casual, uncoordinated, and almost accidental way, reflecting the individualism and suspicion of centralized organization shared by most Universalists who felt sufficiently strong in their religious convictions to join a distinctive group” (p. 56). Its relationship to Unitarianism was strained during the first half of the last century; the theological emphases of these two liberal bodies were not identical (Universalism saw itself theologically between Unitarianism and the Evangelical churches), and Universalism was more a church of the people, while Unitarianism was strong among the upper classes. But—and here Miller challenges a long-standing generalization—Universalism was not primarily a rural movement in its early years. It was “confined at first largely to urban centers, and did not spread to the interior until after 1820 as part of general westward migration” (p. 161).

Many other facets of the institutional, educational, publication, and reform history of Universalism are traced in leisurely detail. For example, the church played a pioneering role in the ministry of women: “There seems to have been an unusually large number of individuals, both men and women, who became champions of the rights of women and/or made significant social contributions” (p. 535). Clara Barton and Mary A. Livermore became widely known, while Olympia Brown (1835–1926) was the first woman to receive fully recognized ordination in America (1863). Stronger on institutional than on theological history, this book makes an important contribution to liberal denominational history.

ROBERT T. HANDY  
*Union Theological Seminary,  
 New York*

MASAO MIYOSHI. *As We Saw Them: The First Japanese Embassy to the United States (1860)*. Berkeley and Los Angeles: University of California Press, for the Center for Japanese and Korean Studies, University of California, Berkeley. 1979. Pp. xi, 232. \$14.95.

In 1860, the Edo Bakufu dispatched a group of over one hundred seventy men to the United States for exchange of the ratifications of the American-

Japanese Treaty of 1858. In this monograph, Masao Miyoshi, professor of English at Berkeley, narrates the engaging activities of these men who constituted the first Japanese embassy to the United States, analyzes their “minds,” and concludes that “America and Japan . . . lie as far apart now as they did a century ago.”

From the standpoint of historiographical analysis, the contents of the monograph may be divided into three parts: (1) well-known events of the Bakumatsu and Meiji eras that are skillfully presented to provide appropriate historical contexts in which the activities of the embassy and subsequent incidents may meaningfully be described, analyzed, or appreciated; (2) a reconstruction of the men’s perceptions of America and Japan; and (3) an account of the lives of the men after their return from the trip.

Of the three parts, the second is the most valuable. Here one finds a richly detailed and exquisitely sensitive treatment of several timeless patterns of Japanese behavior and thought: the concept of meticulously formulated hierarchy, the Japanese group orientation, the lack of self-assertion in the diaries of the embassy, and the uniformity in the content and style of Japanese writing. With great sensitivity and understanding as well as detachment, the author has elucidated these patterns as underlying the Japanese perceptions of America. It is rather sobering to realize that in general these patterns still help shape the Japanese image of the West.

Those portions of the book, especially the first chapter, that make up the first of the three parts are lucidly written but contain a number of assertions that today’s historians would have difficulty accepting (pp. 13, 15, 16, 17, 21, 22, and 101). As examples, two will be cited. First, the definitions of *fudai* and *tozama* daimyo as given in this book are like those given by the late James Murdoch (pp. 15f). Second, the author asserts that the *sankin kōtai* system required the daimyo to reside in Edo “every half-year” (p. 101). It is well known that a small number of daimyo did so, but the bulk of them resided there every other year.

The last of the three parts, taking up as it does 42 of the total 186 pages of the text, adds little to the content of the book. To begin with, the biographical sketches of many lives are too short to be meaningful, while the fairly lengthy accounts of two lives, Katsu Kaishū and Fukuzawa Yukichi, just present well-known facts. Beyond this, by his own admission, the author merely has presented “a pastiche of some of the careers . . . ignoring the explicit cause-effect readings of the voyage and the later lives” (p. 154).

The monograph is well worth reading on account of the second part alone. In both style and content the book has great appeal to scholars and laymen

alike. It could have easily been brought out by such publishers as Kōdansha as well as by any university press.

RICHARD T. CHANG  
University of Florida

DAVID J. WISHART. *The Fur Trade of the American West, 1807-1840: A Geographical Synthesis*. Lincoln: University of Nebraska Press. 1979. Pp. 237. \$15.00.

In the considerable body of literature that exists on the American fur trade of the trans-Missouri West, the traditional focus has tended to be romantic and personalistic. Emphasis on the heroic confrontation between nineteenth-century man and the natural environment has dominated this literature; there has been confusion between real and apocryphal, personality has overshadowed place, and the real or perceived brilliance of individual exploits have hidden the system within which these individuals operated. There are notable exceptions to these criticisms of the genre in the work of L. R. Hafen, H. M. Chittenden, P. C. Phillips, and J. E. Sunder. To this list must now be added the name of David J. Wishart whose *Fur Trade of the American West, 1807-1840: A Geographical Synthesis* provides a new interpretation of the fur trade, viewing it as a system operating in space and time. In this most interesting work, written by a historical geographer, the guiding ideas were what D. W. Meinig, also a historical geographer, has referred to as "strategy and ecology—man's organization of area and man's relationships with the other parts of the ecosystem."

Wishart prefaces his study with an analysis of the geographical setting of the fur trade, a setting that consists of the geostrategical context of the trans-Missouri region as well as the physical and biological characteristics of the landscape. Then, following the guiding principles of his work, Wishart analyzes the "strategy" of the Upper Missouri fur trade in bison robes in terms of human organization of the area of the Upper Missouri basin and discusses the "ecology" of the trade by describing the relationships between the traders and the other elements in the ecosystem (including native Americans) in terms of the annual cycle of operations. Similar discussions of strategy and ecology are implemented for the more familiar Rocky Mountain fur trade devoted largely to beaver pelts. The author then concludes his analysis by assessing, in ecological terms, both the Upper Missouri and Rocky Mountain components of the system as parts of a greater whole, the totality of the trans-Missouri fur trade. The result of this assessment is a decidedly anti-romantic image of the fur trade as ecologically destructive to the physical environment and the native inhabitants of the area alike.

Like many innovative works, the book suffers from occasional inconclusiveness. But the research work has been done carefully and Wishart's conclusions on the fur trade as "an early stage in the progressive dissipation of the American environment" are certainly thought provoking in light of the emerging interest in environmental history. But whether it is better (as Wishart claims) to view the fur trade through the "dark lens" of environmental destruction or through the "rosy lens of frontier romanticism" remains moot. Perhaps viewing through any single lens exclusively is dangerous, and, although Wishart's bias does not seem to be an illogical or unsubstantiated one, those engaged in the fur trade *were* figures of romance in a romantic period. To ignore that fact altogether in favor of a completely "systematic" argument does not seem totally appropriate.

JOHN L. ALLEN  
University of Connecticut,  
Storrs

MARTHA ROYCE BLAINE. *The Ioway Indians*. (Civilization of the American Indian Series, number 151.) Norman: University of Oklahoma Press. 1979. Pp. xiv, 364. \$24.95.

Martha Royce Blaine's work is an ambitious ethnohistory of the Ioways. She traces the tribe chronologically from earliest prehistory in northeastern Iowa to final allotment in Oklahoma during the last quarter of the nineteenth century. Between those times, Blaine states, the Ioways were a force in the region during the seventeenth and eighteenth centuries. Information detailing the tribe's activity during this period is sparse and vague, despite apparently frequent contacts with French, Spanish, and English fur traders. The crossroads location of the Ioways' territory between the Missouri and Mississippi Rivers not only affected their role in the fur trade but also brought them into sustained contact with other tribes inhabiting the region. Eventually that same pivotal position led to intercourse with the Americans, particularly after 1800. A predictable course of events—loss of territory and tribal sovereignty, economic dependence, and cultural devastation—followed. Blaine contends that the Ioways exerted a regional influence far greater than their modest numbers would suggest. The tribe exercised this influence due primarily to the fortuitous location of their homeland—a location that fostered a significant measure of "middleman" activity in the fur trade, thus ensuring the tribe's position as a regional force well into the nineteenth century.

Blaine's analysis, however, is not completely supported by the evidence she presents. The tribe's im-

portance during maneuverings by the European colonial powers is outlined, although the Ioways appear only tangentially important in regional affairs until the American influence began to be felt around 1800. The sketchy nature of the information known about the Ioways during the pre-1800 period is stretched precariously thin to establish the tribe's importance during this period. As such, it seems an inadequate basis upon which to draw such significant conclusions. Moreover, it is questionable whether the Ioways in the post-1800 period, fragmented as they were into at least two opposing factions whose total population never exceeded 1,500 and buffeted by a host of other Indian tribes, would have been in any position to exert a dominating influence in the region or the fur trade.

The main contribution of Blaine's work rests with the straightforward, factual ethnohistory she presents of the Ioways. Drawing upon a wide variety of primary and secondary sources, Blaine has brought this material together into a useful narrative. In several instances, however, the narrative is fragmented and plodding due to the scarcity of source material or the inclusion of irrelevant information that is a distraction to the reader. Nevertheless, this work will be a useful addition to the study of Native American ethnohistory.

RONALD RAYMAN  
Western Illinois University

DANIEL F. LITTLEFIELD, JR. *Africans and Creeks: From the Colonial Period to the Civil War*. (Contributions in Afro-American and African Studies, number 47.) Westport, Conn.: Greenwood Press. 1979. Pp. xiii, 286. \$22.50.

It is impossible to understand the history of the Five Civilized Tribes without attention to the black men, as slaves or as freedmen, in their midst. In recent years, thanks to the studies of Daniel F. Littlefield, Jr., and others, that story is becoming better known. Littlefield began his work with *Africans and Seminoles: From Removal to Emancipation* (1977), continued it with *The Cherokee Freedmen: From Emancipation to American Citizenship* (1978), and now in the present work considers the presence of blacks among the Creek Indians.

The Creeks, like others of the southern tribes, had no well-developed slavery system of their own, although there were occasions when other Indians were enslaved. But as their contacts with whites multiplied and as increasing numbers of black slaves fled into the Creek Nation, the Creeks adopted the white practice of black slavery. From the Revolutionary War on, blacks became an important element in Creek history. They affected the economic development of the Creeks, for many

Creeks (especially mixed bloods) adopted a plantation economy, and they seriously complicated the relations of the Creek Nation with the whites and with neighboring tribes. Although Littlefield supplies a detailed section on the nature of Creek slavery and the slave codes that enforced it, he is more concerned with recounting the presence of the blacks in the general unfolding of Creek history.

Creek relations with blacks were intertwined with Creek-Seminole relations, for slaves of the Creeks as well as of white planters fled to Seminole country, where their bondage was generally light. Many blacks among the Seminoles led almost independent lives and some, because of their knowledge of English, served as interpreters and often had great influence on Seminole affairs. When the Creeks and Seminoles were closely associated in Indian Territory in the West, the problem of the free or nearly free blacks among the Seminoles caused innumerable conflicts, in which officers of the United States government often intervened. The book makes constant reference to the Seminoles, and substantial sections are as much about them as about the Creeks. In fact, many pages repeat almost verbatim long passages from *Africans and Seminoles*.

The story told is an important one, and the book reflects extensive research in archival documents. The presentation, however, is faulty, for the book seems to have been written in haste without adequate digestion of the sources. There is a great deal of anecdotal reporting—great detail of names and places and minor events that illustrate but do not advance the action—and an annoying technique of supplying long lists of names of slaves both in the text and in the notes. The reader who can ignore such irritations, however, will begin to get an appreciation of how significantly blacks fitted into Creek and Seminole history in the decades before the Civil War.

FRANCIS PAUL PRUCHA  
Marquette University

RONALD L. LEWIS. *Coal, Iron, and Slaves: Industrial Slavery in Maryland and Virginia, 1715-1865*. (Contributions in Labor History, number 6.) Westport, Conn.: Greenwood Press. 1979. Pp. 283. \$22.95.

Joining the growing rivulet of scholarship directed in recent years toward the industrial employment of slaves in the plantation economy, this concise, detailed work, a revised and updated dissertation, demonstrates the worth of such investigations in comprehensively understanding the peculiar institution's operation in the Old South. This study focuses upon the labor employed from the colonial period through the Civil War in the eastern Virginia bituminous coal field and in the various iron-

works in the Baltimore, Maryland, vicinity, Richmond, and the upper Valley of Virginia. At various times, approximately 4,500 to 7,000 bondsmen labored at the numerous Maryland-Virginia ironworks and between 500 to 1,000 toiled in the cyclical Virginia coal industry.

Recognizing the slaves' substantial influence on whites to adjust labor policy, the author finds the relationships between slaves and masters in the ironworks and coal mines to be based on forced compromise rather than on absolute authority. What emerged was a three-way relationship: (1) the slaves pushed for advantages within certain parameters; (2) the employers yielded without losing ultimate control; and, (3) the slaveowners attempted to protect and profit from their property. These industrial employers of slaves pursued a policy of compromise towards bondsmen, not because of humanitarian considerations but for more pragmatic reasons. The owners and managers used negative and positive incentives to motivate their work force. Negative incentives included verbal chastisement, placement in more difficult tasks, whipping, and threats to sell, whereas positive stimulation arose from granting of free time, manipulation of food and clothing allotments, monetary remuneration, and the overwork system. Despite all motivational techniques, many slaves in both industries never accepted their plight and combated it by excessive drinking, theft, escape, and temporary disappearance.

By suggesting that profitability in the coal and iron industries might be more dependent upon managerial considerations than upon the labor system, Lewis deftly avoids the final answer to the question because of the lack of data. Other factors such as marketing, location, adaptation to technological innovation, transportation costs, and the risky nature of coal mining played more deterministic roles in ultimate success of the enterprises. It is certain that the entrepreneurs concerned preferred slave labor and thought it to be superior to white labor. Throughout the period, these iron masters and coal operators, like their plantation brethren, opposed a social, political, and economic system that benefited industrialization. Although temporary differences arose over the issues of tariff and industrial self-reliance, they, being social conservatives, preferred the plantation society based upon a predictable and stable labor system and upon a common understanding of communal responsibility in continuance of a slave system.

The major strength of this study is its employment of thorough research to document the day-to-day workings of industrial slavery and the hire-system in major Old South enterprises without resorting to questionable theorizing and dubious generalizations. Lewis reaches temperate conclusions,

buttresses his presentation with numerous tables, realistically employs his evidence in relating the contemporary operational environment of these dynamic industries, and successfully marries his findings with the most recent scholarship in the field. More than a labor study, the volume traces the rise of the respective enterprises, accounts for their importance, and places their development into a national economic context.

JOHN E. STEALEY III  
Shepherd College

MARCUS CUNLIFFE. *Chattel Slavery and Wage Slavery: The Anglo-American Context, 1830-1860*. (Mercer University Lamar Memorial Lectures, number 22.) Athens: University of Georgia Press. 1979. Pp. xix, 128. \$9.00.

Marcus Cunliffe delivered the three lectures that compose this book as the Lamar Memorial Lectures at Mercer University in 1978. Since the lectures' purpose is to explain and to preserve Southern life and culture, the selection of Cunliffe, the first non-American and nonspecialist in Southern history, seems, at first glance, surprising.

Cunliffe justifies the faith of those selecting him, however, choosing to lecture upon the history of a controversy over the relative merits of wage slavery in England when compared with chattel slavery in the United States. Beginning with George Fitzhugh, whom he calls the pioneer in the comparison of the two institutions and describes as "wiser than either the abolitionists or the complete defenders of the status quo," Cunliffe ends with a discussion of the ideas of Charles Edward Lester, an obscure Northern clergyman-abolitionist who migrated to England in 1840, as primarily expressed in *The Glory and Shame of England*. The book, published in 1841, reflected Lester's experiences in the North and in England and commented caustically upon wage slavery in England. In the intervening lecture, Cunliffe describes the mutual criticism aimed at each other's societies by intellectuals on either side of the Atlantic. Americans, convinced that English social organization exploited the workers, believed the English criticized America's peculiar institution in order to distract attention from that organization and to "undermine" the American Union. The English, on the other hand, regarded American society as vulgar, corrupt, dishonest, and, most particularly, hypocritical in its profession of freedom while practicing slavery.

The exposition is a graceful one displaying a certain familiarity with what Cunliffe admits must be considered a minor strain in Anglo-American thought. Although he asserts that the attacks on wage slavery made by Lester and Fitzhugh repre-



sented opinions more commonly held than has previously been supposed, he fails to substantiate fully that assertion in this microcosmic study. Nor does his conclusion that our generation has accepted the view that chattel slavery was "more economically viable and more robustly absorbed by its black force" than its opponents realized seem justified when the reaction to that view has been as heated as it has.

While containing little new for students of slavery, the book does show how well Cunliffe met the purpose of the series, to provide lectures of the highest quality to the public.

DWIGHT HOOVER  
Ball State University

DICKSON D. BRUCE, JR. *Violence and Culture in the Antebellum South*. Austin: University of Texas Press. 1979. Pp. x, 322. \$16.95.

In the antebellum South, "it was remarkable, and worrisome, how many gentlemen were prepared at all times to turn a minor altercation into a killing" (p. 72). Within the pre-Civil War South there was overwhelming acceptance of violence as being unavoidable. Yet, Southerners strongly cherished order; for them discipline was fundamentally important, so much so that in their child-rearing practices they attempted to eliminate selfishness from their children.

Southerners viewed the world as one of violence, a theme Dickson D. Bruce, Jr., explores by focusing on the meaning of violence, its sources, and its implications. Following an excellent introduction, he devotes chapters to the Southern duel, child rearing, views of the planter-class toward violence, plain-folk society, violence from both the slaver's and slave's point of view, militarism, fiction, and a conclusion centered around the relevance of Edgar Allan Poe to a discussion on Southern violence. "Poe," Bruce writes, "expressed the logical outcome of many Southerners' world view in his tales" (p. 240), which portrayed human nature as perverse, often dominated by evil.

Bruce believes "the basis for Southern attitudes toward violence lay in pervasive patterns of culture that extend well back into Antebellum history" (p. 8). For Southerners the villain that gave rise to violence was passion. Viewing passion in negative and destructive terms, most Southerners distrusted human nature; at best, they could only hope to keep passion under control. With slavery in their midst, the concern Southerners had about the need to control violence was exacerbated. Given such an institution, built and maintained through violent force, fear of slave uprisings dominated their thought. The mere idea of abolition also evoked fear of vio-

lence: "abolitionists, or suspected abolitionists, were the most common targets of lynch law" (p. 130). "Violence," contended Frederick Douglass, "not morality, was the language of American race relations" (p. 159).

Craving order but convinced it was fickle and unlasting, Southerners viewed violence as the opposite of order. Paradoxically, while placing a premium on order, the most figurative language used by them was violent. The "source which provided the greatest stock of Southern orators' violent imagery was the American Revolution" (p. 184).

Leaving in place the general view that antebellum society was the most violent within the nation, Bruce theorizes that Southerners' fear of passion lay at the heart of the problem. Their obsession with the need to control passion interacted with their general pessimistic attitude to produce within them an overall violent temperament, one even conducive to civil war. Insecure within their racist and stratified sectionalism, with a national creed proclaiming freedom and equality, antebellum Southerners were frightened people. They saw or imagined threats and dangers everywhere, even within themselves. Bruce does not say it, but the legacy of that disposition haunts our own times.

E. C. FOSTER  
Jackson State University

RAY MATHIS. *John Horry Dent: South Carolina Aristocrat on the Alabama Frontier*. University: University of Alabama Press, for the Historic Chattahoochee Commission. 1979. Pp. xiv, 267. \$17.50.

In 1837 when he was twenty-two years old, John Horry Dent left his economically depressed home state of South Carolina and established his young family in Barbour County on the Alabama frontier. Drawing on Dent's extensive collection of farm journals, diaries, financial records, and short essays, Ray Mathis has created a detailed and rich mosaic that illustrates his subject's rise as a prosperous planter and businessman. The result is a valuable microcosmic slice of Old South life that allows the testing of many of our large historical assumptions against individual experience.

The book's principal strength is also its main weakness. It is perhaps half-authored and half-edited, since Mathis quotes extensively from Dent's journals, allowing the subject most often to speak for himself. Although Dent was consistently detailed and competent, he was rarely eloquent; thus, the narrative sometimes takes on a tedious quality for the reader. Nonetheless, for the specialist willing to plow through these passages, there are glittering rewards: a full, clear account of the plantation year, the month-by-month agricultural operations in



which Dent engaged; a good exposition of this paternalist master's attitudes toward and treatment of both slaves and overseers; a compelling depiction of Dent's unflattering obsession with economic gain as a key to social status; and a thoughtful treatment of plantation adjustments with the advent of black emancipation.

Mathis's monograph deals only with Dent's life and occupation through the end of the Civil War, leaving for subsequent treatment his career as an agricultural reformer, farmer, and financier in north Georgia. Nor is the volume much concerned with local history or Alabama politics. Historians of the Old South, especially those interested in agriculture and economics, will find John Horry Dent an informative acquaintance.

HUGH B. HAMMETT  
*Empire State College,  
 State University of New York*

JOHN MCCARDELL. *The Idea of a Southern Nation: Southern Nationalists and Southern Nationalism, 1830-1860*. New York: W. W. Norton and Company. Pp. xi, 394. \$16.95.

This Allan Nevins Award winning work ambitiously seeks to explain the origins and development of the ideology of Southern nationalism. However, despite, or perhaps because of, its ambitious scope, John McCardell's study is disappointing in that it too often amounts to a bland synthesis of existing scholarship. What the reader searches for in vain are those "fresh conclusions about the nature of the secession movement" promised in the introduction (p. 8).

To be sure, the general reader and the undergraduate student in Southern history will find much of value in the narrative. In a series of topical and biographical chapters focusing on the period after 1830, McCardell touches base with that variety of strains—social, economic, and political—which comprise the familiar backdrop to the republic's intensifying sectionalism. Sound and generally insightful discussions, based on the latest scholarship and often on the imaginative use of manuscript sources, are presented in such areas as proslavery thought, literature, and expansionism. Still, in common with most works of synthesis, the text lacks depth and nuance. At times the very breadth of the material that McCardell feels compelled to cover results in superficiality, as in his treatment of late antebellum Southern religion that glosses over both the ethos of evangelicalism and its social appeal; in misleading generalizations, as evidenced in his conclusion that the "education of young children was hardly neglected" (p. 179); or in a rushed narrative, such as the final chapter on postnullification Southern and national politics.

Had McCardell adhered more closely to the conceptual formulations that he laid out in his introduction, *The Idea of a Southern Nation*, in my judgment, would have been a more successful work. For example, borrowing from Clifford Geertz, McCardell suggests that ideology be approached as an effort to cope with stress in the social structure and cultural belief system. Although the ensuing narrative, especially in its biographical portraits, often locates individual stresses and relates them to a personal commitment to Southern nationalism, McCardell stops short of linking tensions of individual personalities to Southern culture or society as a whole. Indeed, just when such a linkage appears about to be formed, McCardell moves on to another topic or retreats into irony. Thus, at the end of a fine chapter on nullification, he finds "ironic" that the most radical nullifiers, those committed to a Southern nation outside the Union rather than a protected Southern section within the Union, were "entrepreneurial, parvenu planters" (p. 48) who perceived the North as a threatening society of venal, money-grubbing shopkeepers. The irony dissolves when we consider that the emerging Southern nationalists, and putatively Southern culture as a whole, when confronted with unavoidably contradictory expectations of identity—gentility versus aggressiveness, chivalry versus materialism, order versus competitiveness—displaced onto the mythical Yankees those values and personality traits that they so hated in themselves and then in turn used that displacement as a justification for separating from the North.

Though one is left unsure as to the extent of Southern nationalism, even by 1860, I would recommend McCardell's study as a useful survey of the background to secession. As a significant and original contribution to our understanding of that movement, the book falls short.

WILLIAM L. BARNEY  
*University of North Carolina,  
 Chapel Hill*

WALTER EHRLICH. *They Have No Rights: Dred Scott's Struggle for Freedom*. (Contributions in Legal Studies, number 9.) Westport, Conn.: Greenwood Press. 1979. Pp. xvi, 266. \$22.50.

Walter Ehrlich's *They Have No Rights* will doubtless be referred to as "the other" modern treatment of the *Dred Scott* case. Clearly overshadowed by Don Fehrenbacher's deep and wide-ranging analysis of the same general subject, this narrower study develops narrative legal history almost exclusively. Ehrlich, unlike Fehrenbacher, offers no sustained analysis of antebellum racial feeling, political ideologies, and social stresses or their impact upon the thinking of lawyers and judges. He also does not consider the

complicated consequences of the decision itself in the final years of the sectional conflict, from 1857 through 1861, or its broad impact on race and law in the post-Civil War era. Instead, Ehrlich pursues the more restricted but nevertheless worthwhile task of tracing Dred Scott's lawsuits from the mid-1840s onward in order to demonstrate how they evolved from straightforward pleas for an individual's emancipation into devices by which jurists and politicians addressed the deepening crisis over slavery's expansion. To this end, Ehrlich has carefully examined legal briefs, judicial pronouncements, and press accounts in order to set forth in his words "the detailed morphology of the case itself" (p. xiii). Historians of law and of the sectional conflict now possess a reliable, modern, and even absorbing brief account of how Dred Scott's quest for freedom slowly became a political *cause célèbre*.

Ehrlich's approach demands that he concentrate heavily on the early phases of Scott's life and litigation. Nearly half the book involves detailed examinations of Scott's situations and travels in the 1830s and 1840s, the career and personal histories of his various owners, and the motives of those who became his early advocates. It is a tangled story, complicated by conflicting interpretive possibilities and ambiguous evidence. But Ehrlich employs careful judgment and straightforward prose to establish very clearly that Scott's case took its initial form without reference to larger slavery issues. Litigants at first perceived no broader sectional implications, but as appeals went forward, lawyers and jurists began to expand the questions involved and make them even more controversial. The final case of *Dred Scott v. Sanford*, it is clear, was in no way the product of conspiracy. Yet Ehrlich's treatment certainly clarifies why it was so widely perceived as such; his "morphology" demonstrates how the people involved in *Dred Scott*'s development actively reformulated the case into a wide-ranging vehicle for sectional expression. By treating lawyers and jurists as responsible historical actors, while de-emphasizing the analysis of abstract legal opinion per se, Ehrlich succeeds in elaborating with useful conciseness and originality how and why the *Dred Scott* case took on the form that it finally did.

The balance of *They Have No Rights* treats the processes of argumentation, consultation, and opinion rendering by which the Taney court disposed of *Dred Scott*. Readers already acquainted with modern scholarship will find in these chapters a useful analysis of familiar themes, and will especially appreciate Ehrlich's explanation of newspapers' roles in shaping popular reaction to the Supreme Court's final decision and published opinions. Yet compared to Fehrenbacher's magisterial work, Ehrlich's brief treatment of *Dred Scott* may seem less substantial than it actually is. By restricting his scope and without advancing inflated claims, Ehrlich has suc-

ceeded in rendering a clear, developmental account of the *Dred Scott* case as it progressed through the courts to its final but disruptive adjudication.

JAMES BREWER STEWART  
Macalester College

MECHAL SOBEL. *Trabelin' On: The Slave Journey to an Afro-Baptist Faith*. (Contributions in Afro-American and African Studies, number 36.) Westport, Conn.: Greenwood Press. 1979. Pp. xxv, 454. \$25.00.

Mechal Sobel, who teaches history at the University of Haifa in Israel, has contributed in *Trabelin' On* an important survey of the church life of black Baptists in the antebellum South. Unlike most treatments of slave religion, this book focuses on the "visible institution" of quasi-independent black churches, which, Sobel demonstrates, were more numerous and widespread than previously thought. Gathering data from church minutes and association records, she lists and briefly describes 205 black Baptist churches founded before emancipation. Of these, 130 existed in the South. Sobel also narrates the activities of black Baptist preachers and sketches the growth of black congregations over time.

Historians of slavery and of American religion will welcome Sobel's data on black churches and ministers. Her explanation of the transformation of slaves from Africans to Afro-Americans is likely to meet with disagreement, however. Although recognizing that there was no single cosmology for West Africans, she asserts that a distinctive "African Sacred Cosmos" did emerge in America. This African Sacred Cosmos began to change as African languages and pidgin English gave way to Creole, which in turn yielded to black English by the third generation of American-born slaves. Sobel's model for cultural change is linguistic: just as slaves moved from African languages and pidgin to Creole and finally to black English, so they moved from an African Sacred Cosmos to African Christianity and finally to an Afro-Christian world view. This model might have been more enlightening if she had probed more deeply the relationship between language and cosmology. As it stands, her analogy is more suggestive than illuminating.

Confronting the related issue of the transmission of African religions in the U.S., Sobel argues that slaves were influenced by African concepts of personality and spirit. In the African concept of person, the soul is a complex or multiple entity. Besides the self there is another inner self, a "little me within me." Experience of the inner self was a common constituent of many ex-slave conversion narratives, and, according to Sobel, the vision of a "little or another me" differentiates black conversion narratives from those of whites. Moreover, the African concept

that spirit originates from the Supreme God and returns to Him continued to inform Afro-Christian visions of traveling to heaven and encountering the Lord. African notions of spirit as animating force, enlivening gods, men, animals, and objects, formed the basis for slave conjuring. There is much that is valid in Sobel's analysis of African influence on slave belief and behavior, although she tends to overstate the case. Comparison with black religions in Latin America might have helped her to see the issue of African influence in broader perspective and might have led her to moderate her views.

On balance, *Trabelin' On* is a valuable source of information about the formation and growth of black churches from 1760 to 1865. And, as the author intends, it is a useful chart for further research into black ecclesiastical history.

ALBERT J. RABOTEAU  
*University of California,  
Berkeley*

DONALD G. NIEMAN. *To Set the Law in Motion: The Freedmen's Bureau and the Legal Rights of Blacks, 1865-1868*. (KTO Studies in American History.) Millwood, N.Y.: KTO Press. 1979. Pp. xvii, 250. \$19.95.

To be enlightened on the history of the Freedmen's Bureau, with its organization of assistant commissioners in the states and its agents in the counties and municipalities of the South, is to be enlightened on the essence of Reconstruction itself. For these men probably knew more about the practical everyday problems of Reconstruction and the prospects for a workable policy than any other group in the country. In a tightly and lucidly written book, based primarily on Freedmen's Bureau records in the National Archives, Donald G. Nieman is indeed enlightening. His book is an important contribution to the history of the Bureau and in turn of Reconstruction.

Although Nieman focuses on legal protection for blacks against white violence, in drawing up and implementing labor contracts, in the freedmen's struggle to retain possession of abandoned lands, and in civil rights, these matters were so central to the operation that the book is in essence a history of the Freedmen's Bureau. In important respects it supersedes George Bentley's work of twenty-five years ago.

Nieman's method is to let the evidence speak for itself, and it does speak, in indubitable tones. What the evidence reveals is a Bureau whose members are frequently—but not always—dedicated to the fulfillment of their charge, but with the odds overwhelmingly against them. This was so for several reasons: a prevailing adherence to states' rights, North and South, in the Reconstruction era that

vastly reduced the effectiveness of the Freedmen's Bureau Bill and the Civil Rights Bill; the absence of enough military to enforce the laws and the reluctance of military leaders to use what power they had; the insurmountable difficulties of operating in an alien region whose unchanging values and social mores underlay both overt and subtle resistance; the current fear of paternalism among Bureau officials of all ranks; and a president who unceasingly worked to undermine the agency. It should be said, however, that Nieman places Andrew Johnson in a proper perspective. While he does not minimize Johnson's role, he makes it clear that this was only one cause, among many, for the Bureau's frustrations and failures.

With originality and keenness of insight Nieman shows some of the ironies in the Bureau's operation: state codes were rewritten, for example, to make them conform to the Civil Rights Bill. But the new, apparently liberal, laws actually weakened the Bureau in dealing with civil rights because cases arising under them could be decided in state courts where probate judges and justices of the peace meted out traditional southern justice to blacks. The Bureau was most effective in enforcing civil rights in Kentucky because that border state did not permit Negro testimony in cases involving whites. It has long been known that the Reconstruction Acts of 1867 and 1868 did not revolutionize the South, but it is surprising to learn how little difference these laws made to the Bureau or the freedmen it was trying to protect.

Quietly and without polemics, Nieman's book suggests that the "retreat from reconstruction" was not merely a phenomenon of the 1870s but that it began very early and at the grassroots of southern society. "In the face of massive resistance from white southerners," he writes, "a temporary and poorly staffed agency which possessed only limited authority could not perform the herculean task of providing blacks with a firm basis for freedom" (p. 222).

*To Set the Law in Motion* is the first book of a promising young scholar. Let us hope that he will be encouraged by his success to continue his scholarly efforts.

PATRICK W. RIDDLEBERGER  
*Southern Illinois University,  
Edwardsville*

ERNEST WALLACE. *The Howling of the Coyotes: Reconstruction Efforts to Divide Texas*. College Station: Texas A&M University Press. 1979. Pp. xii, 217. \$15.75.

When Texas entered the Union, in 1845, it claimed an area that could have been divided into 312 states of the size of Rhode Island. According to the joint

resolution for annexation, Texas had the privilege of dividing itself, not into 312 states, but into as many as five. It gave up more than a fourth of its territorial claim in the Compromise of 1850, but it retained so vast and so diverse an expanse that, from time to time, a further cession or a division made sense to some of its citizens. During the next decade and a half they proposed various and conflicting plans for reducing its size or for cutting it up into two or more states.

Divisionists came the closest to success with the onset of Radical Reconstruction. The Radical Republicans, in Congress and in the Texas constitutional convention, were now the most enthusiastic advocates of partition. Thaddeus Stevens liked the idea, and the Joint Committee on Reconstruction approved the Beaman bill, which would have carved Texas into three parts, but which never got through Congress. The favorite scheme of Texas Radicals would have created a new state of West Texas and would have admitted it into the Union while leaving the rest of Texas unrepresented and under military occupation. In 1869, a draft of a constitution for the new state was published as a pamphlet (of which only a half-dozen copies are known to have survived). Even the proponents could not agree on the boundaries, however, and opponents derided the "State of Coyote" and "the howling of the coyotes" who wanted it. The controversy prolonged the convention and helped to account for the delay in the readmission of Texas. "Since 1869, the 'Coyotes' have been heard occasionally on the western horizon, but the noise has been from a lonely few baying at the moon rather than from a ravenous pack intent on tearing to pieces its helpless victim" (p. 137).

Though historians of Texas have made passing reference to divisionist movements, Ernest Wallace is the first to undertake an exhaustive treatment of the theme. His book is rather brief, with only about 144 pages devoted to the history of the subject and with 46 given over to an appendix reprinting the Constitution of the State of West Texas. Still, his account is authoritative and detailed enough to be called definitive. Nine maps show the outlines of West Texas and other proposed new states. The book will be of interest primarily to aficionados of Texas history, secondarily to students of Reconstruction in general.

RICHARD N. CURRENT  
*University of North Carolina,  
Greensboro*

RONALD T. TAKAKI. *Iron Cages: Race and Culture in Nineteenth-Century America*. New York: Alfred A. Knopf. 1979. Pp. xviii, 361. \$15.95.

Over the past decades, historians have made numerous efforts to analyze the racial thinking of white Americans toward blacks, Indians, Chinese, and other "coloreds" of the world. With few exceptions, these efforts have tended to focus on one race at a time. Ronald T. Takaki has attempted to broaden our range of vision by seeking areas of commonality that encompass all of white America's race attitudes. He has recognized the multifaceted nature of this task—the need to examine racism against the background of broadly defined political, social, and economic forces—and has convincingly extracted the racial mood of white Americans from the ideologically murky waters of the nation's self-portrait. To be sure, this was no easy task as it required Takaki to stalk profanely through the very sanctum sanctorum of American beliefs. Utilizing a broad array of sources—from Benjamin Rush and Thomas Jefferson to Bret Harte, Henry George, Alfred T. Mahan, and others—he has examined those values, images, ideas, and assumptions held or shared by the "culture-makers" and has attempted to tie racial thought to a broader spectrum that included the Protestant ethic, principles of self-government, nationalism, and individualism. Borrowing from both Marx and Melville, he concludes that nineteenth-century white Americans "mutilated" the full potential of their ideals by allowing racial oppression to feed on the very forces that made America into a world power. Each phase of America's transformation from an agrarian-commercial economy to a technological and bureaucratic society has its own "iron cage" within which society defined, controlled, and projected its image. And within each cage, Americans played out a role that integrated both the demon and the divine, or, to borrow from D. H. Lawrence, "clever America lies on her muckheaps of gold, strangled in her own barbed wire of shalt-not ideals and shall-not moralisms."

What Takaki has undertaken is ambitious by any standard, although there is certainly nothing novel in the attempt. In recent years historians have taken great pains to dissect the dynamics of American politics and the paradox of its democratic ideology. Some of the most exemplary thinking among white Americans in their effort to assert democratic ideas is ironically wed to racial attitudes of the basest sort. The reality of this paradox brings one in full circle (Melville would have been proud), for the very term "freedom" acknowledges an irksome internalized conflict that casts shadows across its broadest interpretation. The "colored" races stood at arm's length from the body politic and had to be made safe before being made democratic.

Over and over again, one is impressed with the breadth of Takaki's analysis. Yet there is room for even greater breadth. His American character-



izations of the Chinese and Negroes are strikingly reminiscent of the depictions given by Carl von Linnaeus in his *Systema Naturae* (1735), and G. W. Cable's portrayal of blacks as a race under the influence of the instinctual life had a "scientific" origin that extended backward through the centuries among European savants. Had Takaki read more in the medical and scientific literature of the era, he probably would have had to expand his theme to encompass the self-images of Western culture as opposed to simply American society. Attitudes on women's inferiority that he attributes to American physicians was part of a much larger anthropological and medical background with origins deep in European thinking. The anthropometry of race and sex has a most fascinating history with the authorship of studies on the brain weight of women and the mental differences between the races and sexes bearing no one stamp of nationality. Despite these deficiencies, *Iron Cages* is a thoughtful and important study and deserves careful reading by historians and social scientists.

JOHN S. HALLER, JR.  
Indiana University Northwest

ROBERT C. BANNISTER. *Social Darwinism: Science and Myth in Anglo-American Social Thought*. (American Civilization Series.) Philadelphia: Temple University Press. 1979. Pp. 292. \$17.50.

Robert C. Bannister reassesses themes Richard Hofstadter pioneered in *Social Darwinism in American Thought* (1944). Here Bannister is only concerned with examining the impact of those specifically "Darwinian concepts of struggle for existence, natural selection, and survival of the fittest" (p. 7), not with evolutionism more generally. Bannister insists that Hofstadter was wrong to argue that a powerful conservative Social Darwinist movement justified the status quo in the Gilded Age and sanctified racism, imperialism, and militarism after 1890. Indeed, Darwinism repelled conservatives because of its competitive and relativistic implications. Bannister's thesis is that reformers and liberal academics created a "myth" of conservative Social Darwinism for their ideological purposes. Bannister effectively shows that Gilded Age conservatives often labeled Social Darwinists did not fit his definition of the term. Such conservatives as William Graham Sumner, Herbert Spencer, and Andrew Carnegie wanted "stability, consensus, homogeneity, and peaceful change under a capitalist regime"; therefore, they found "little comfort in a cosmology that posited permanent struggle as the engine of progress" (p. 136). They retained their faith in classical economics, "Lockean liberalism," and a "Franklinian" success mythology (p. 136). Bannister

concedes they were evolutionists, but Lamarckians, not Darwinians. Then Bannister traces the emergence of the myth of conservative Social Darwinism from the 1890s to the 1940s on two levels simultaneously: certain post-1890 intellectuals usually considered Social Darwinists—eugenicists, racists, Nietzsche's followers, certain literary naturalists, and foreign policy commentators—who were not Social Darwinists by his definition; and how reformers and liberal academics invented the myth of Social Darwinism as a weapon to use against their conservative opponents in public politics.

No brief review could treat Bannister's intelligent book adequately. His discussions of particular individuals appear generally accurate, and his thesis of the "myth" of conservative Social Darwinism as he defines it is a useful contribution. If his discussion of Gilded Age conservatives amply demonstrates that they were not "Social Darwinists," this is a valuable but not especially new characterization. Bannister seems unaware that Lamarckianism could be and was used by conservatives to justify the status quo. That Bannister found so few *Darwinian* social thinkers is less astonishing when it is understood that many American scientists resisted Darwinian selectionism until the 1920s. Bannister's grasp of science and its history is decidedly superior to Hofstadter's, but it can be criticized here and there, as, for example, when Bannister correctly summarizes the popular misconception of August Weismann's scientific ideas circa 1900 but unfortunately shares that misapprehension. And Bannister ignores the new scientific hereditarianism after 1900 (as, for example, mutationism and mental testing) that lent fresh scientific legitimacy to eugenics and immigration restriction and created racial mental testing—perhaps it was these socially divisive developments Hofstadter mislabeled "Social Darwinism." Finally, Bannister's style is often unnecessarily difficult, and his discussions of individuals and themes after 1890 are often too brief, as, for example, his treatment of the Social Darwinist myth after World War I, which is lamentably reduced to an abbreviated epilogue.

Yet on balance Bannister has added to the general understanding of "Social Darwinism."

HAMILTON CRAVENS  
Iowa State University

SUSAN ESTABROOK KENNEDY. *If All We Did Was to Weep at Home: A History of White Working-Class Women in America*. (Minorities in Modern America.) Bloomington: Indiana University Press. 1979. Pp. xx, 331. \$17.50.

Another in a series of studies of minorities in modern America, this work presents a succinct portrait



of white working-class women from the colonial period to the 1970s. The colorful title is taken from the 1902 protest of Jewish women in New York's East Side, who organized a boycott to protest the rapidly rising price of kosher meat. The boycott turned into a riot, and the police arrested dozens of women. When Rebecca Ablowitz was asked by a judge why she had taken part in the attack on butcher shops, she told him: "We don't riot. But if all we did was to weep at home, nobody would notice it; so we have to do something to help ourselves."

This incident is related (p. 65) as part of a section dealing with immigrant women from several cultures. Much of the terrain covered up to this point in the book has been previously mapped by historians, and while Susan Estabrook Kennedy's treatment is informative, it adds little to what is already known. While it may be unfair to criticize the author for this failing in so short a study, one can still fault her for adding little, and sometimes less in terms of certain key episodes, such as rise and decline of the Female Labor Reform Associations of the 1840s. There is also a tendency in these early sections for too much facile generalization and too little depth of analysis.

The book picks up momentum during discussions of World War I and World War II as Kennedy clearly demonstrates that both of these conflicts held out only a glittering promise of economic and social advance for working women. These sections are a welcome addition to the literature of the period. They furnish valuable information rarely to be found in most standard histories of the United States.

I wish the author had maintained these standards in her discussion of the late 1920s and early 1930s. One would not know from this work that the period between 1926 and 1930 saw some of the fiercest battles involving white women workers. In 1926, a militant strike broke out in the silk mills of Passaic, New Jersey. Two years later, over twenty-six thousand cotton mill workers in New Bedford, Massachusetts, most of them women, struck. The most famous strike of the period took place in Gastonia, North Carolina in 1929-30. Nearly all of these hard-fought battles were lost. But though bitter defeats, they planted the seeds that were to flower in the 1930s. Again, these strikes were under Communist leadership, and this may be the reason for their neglect in the present work. In general, Kennedy either overlooks or dismisses left-wing contributions to improving conditions to working-class women.

Kennedy also fails to give sufficient credit to the CIO and its efforts to organize all workers and not just the skilled. While it is true that the CIO attached low priority to fighting sexually discriminatory employment and wage scales pervading indus-

try, to equate it with the AFL, as Kennedy does, is truly startling.

Despite these flaws, this is basically a solid and useful book and one that reflects at various points the most recent historiographical trends in the field. Its superb bibliography includes an excellent collection of manuscript sources, unpublished studies, and most of the important general works. There is detailed documentation for each chapter and a useful index. In the likely event of a second printing, International Workers of the World (IWW) should be changed to the real name of the organization—Industrial Workers of the World (IWW).

PHILIP S. FONER  
Lincoln University

GILBERT A. HARRISON. *A Timeless Affair: The Life of Anita McCormick Blaine*. Chicago: University of Chicago Press. 1979. Pp. x, 253. \$15.00.

As the McCormick reaper was transforming American agriculture, Anita McCormick Blaine, daughter of its inventor and wife of a son of James G. Blaine, was being transformed along with it. A woman of generous instincts and large perspectives, her lifetime spanned the Chicago Fire and the Korean War, and she used the years in between to make her enormous wealth count for educational, civic, and internationalist advance. *A Timeless Affair* is as much a biography of a family and an era as of an individual and has at its center a woman whose character was fortunately firm. This often turbulent story astonishingly reveals Blaine as able to surmount the religiosity of her mother without retreating into cynicism, to absorb the business distractions of her father without developing filial hostility, to oversee the care of seriously disturbed siblings with minimal resentment, and to endure the premature deaths of a husband and an only child without plunging into any of the ill-advised forms of consolation available to wealthy widows.

A less impressive side of Blaine's life has to do with her many philanthropies. It is true that she never took the predictable posture of philanthropist autocrat. Sensitive to the competing managerial viewpoints that often vied for dominance in the organizations she supported, especially in the progressive Francis W. Parker School, she usually steered a reasonable course and eschewed excessive interference. It is also true that her benefactions and interests were many and varied—the City Homes Association, the Consumer League, Hull House, a Swiss school for the children of League of Nations delegates, the Foundation for World Government, and others. But there appears in her choosing and planning no central philosophy integrating these activities. Her swift acceptance of the ideas of the

renowned student of psychic phenomena, Sir Oliver Lodge, provides a telling gauge of the ease with which her backing could be enlisted. It inevitably compromises the admiration we feel for a millionairess who fearlessly supported Henry A. Wallace and the Progressive Party and later subsidized two newspapers—the *National Guardian* and the *Daily Compass*—intended to promote Wallace's ideas and career.

Gilbert A. Harrison has drawn from a painstaking search through family archives a portrait both vivid and convincing. Blaine's selflessness and her belief in "concession and cooperation" as "preferable to confrontation" (p. 148), for example, are made compatible with her support of Chicago's outspoken union leaders, just as her militant stand against Hitler is seen as an extension of her earlier advocacy of the League of Nations.

Nonetheless, the impression remains of an heiress too isolated from other women to become a feminist, too reluctant to unsettle her comfortable routine to be an activist, and too deficient in serious education to link seasoned intelligence to well-intentioned money.

ANNETTE K. BAXTER  
*Barnard College*

DEE GARRISON. *Apostles of Culture: The Public Librarian and American Society, 1876–1920*. New York: Free Press. 1979. Pp. xvi, 319. \$14.50.

This is an important, well-written, and entertaining book. Dee Garrison traces the development of the public library as one of the many institutions designed to deal with the problems of an urban, industrial America. More important, she analyzes the development of librarianship in the context of the broader professionalization and efficiency movements in American society. But her greatest contribution is the way she relates this professionalization of the library to its feminization. Although the first woman library clerk was not hired until 1852, by 1910, 78.5 percent of all library workers were females. Only teaching and nursing were more feminine in that year, and, by 1920, with 90 percent of all librarians women, only nursing was a more feminized profession.

The professionalization of the library coincided with the arrival on the American scene of college-educated women who "rejected their mothers' role of piety and passivity." Many of these women became librarians, but they rarely rose to the top positions in their profession. In the words of one librarian of Congress, they lacked the "superior traits of men," specifically "manliness" and "a sense of proportion." Instead, they took low-paying jobs and became devoted followers of the male leaders. It was assumed by men and women alike that women

had more patience and more accuracy in details than men and thus could be assigned the tedious job of cataloguing. The nature of library work itself, Garrison argues, served to perpetuate the low status of women in American society. But just as important, "female dominance of librarianship did much to shape the inferior and precarious status of the public library as a cultural resource; it evolved into a marginal kind of public amusement service."

A brief review cannot do justice to this provocative and interesting book. We are introduced to some fascinating characters; John Cotton Dana, William Frederick Poole, Justin Winsor, Charles Ammi Cutter, Mary Bean, Caroline Hewins, and especially Melvil Dewey. Dewey was the principal organizer of the American Library Association, the creator of the decimal system of classification that bears his name, and the originator of schools for library training at Columbia and Albany. Dewey was a mathematical genius who could never keep his own accounts straight; he was an apostle of thrift and efficiency who was beloved by many including a group of devoted women; but he was also hated, and scandal followed him during his frequent moves. Still, this strange and complex man was the single most important force in the development of the modern American library.

This book would be worth reading if only for the chapters on Dewey, but there is much more. Garrison makes a careful analysis of popular American fiction in the nineteenth century and the attitudes of librarians toward that fiction. She makes interesting comparisons between librarians and social workers, and she suggests that World War I and the Library War Service began to change the role of the female librarian and led eventually to the decline of the "genteel library hostess." This book may not please librarians, but historians will find it full of interesting and useful insights.

ALLEN F. DAVIS  
*Temple University*

FREDERICK S. ALLIS, JR. *Youth from Every Quarter: A Bicentennial History of Phillips Academy, Andover*. Andover, Mass.: The Academy; distributed by the University Press of New England, Hanover, N.H. 1979. Pp. xxvii, 770. \$25.00.

Frederick S. Allis, Jr., has declined to write a social history of Phillips Academy, Andover, though he is aware of some recent work following that method. He has written instead a scholarly, institutional history intended both for professional scholars and for an Andover audience curious about their school's past.

Allis follows conventions of other institutional studies, developing themes similar to those of Richard Hofstadter and Frederick Rudolph concerning

colleges and universities. The story of Andover is a tale of progressive reform away from the early "rigid and oppressive insistence on conformity to the old Calvinist creed" (p. 221). He divides Andover's past in half with the turning point coming at about 1870. In the first period, founders established a school to preserve traditional values in a revolutionary society. Between 1810 and 1870, Allis argues, there followed a static period of preserving the ideas of the founders. "The School changed very little" (p. 117). "In its second century it changed markedly" (p. 219). For most of its two centuries, Allis maintains, Andover has remained isolated from social and political changes outside the school. Most of the pressures for change that he discusses came from within Andover.

The book is organized around the administrations of headmasters. It deals with topics familiar in the history of higher education: curriculum, faculty, leading individuals, physical plant, and student life and discipline. It does not explore some significant changes that Allis mentions in passing, like the growth of the student body, its social origins, or influences upon the curriculum from external intellectual currents. Nor does Allis examine ways in which early nineteenth-century Andover was disturbed by changes in New England society, as evidenced by the influx of charity scholars, the wide range of student ages, and the difficulties in housing, feeding, and disciplining students. Identical problems appeared at other schools and colleges. Here Allis's focus on a single institution has exaggerated the sense of Andover's isolation, uniqueness, and static condition.

Allis has made creative use of manuscript material and institutional records in the school collections, and his book demonstrates the richness of these sources. The book raises significant issues concerning the role Andover assumed as a school for an elite. And in treating the great teachers at Andover, Allis has drawn attention to an important question. How did this school continue to demand excellence in teaching, with conscious, critical concepts of method and curriculum? Here has been one key to Andover's distinction. In calling attention to this emphasis, Allis has made his most important contribution.

DAVID F. ALLMENDINGER, JR.  
*University of Delaware*

GERALD MCKEVITT *The University of Santa Clara: A History, 1851-1977*. Stanford: Stanford University Press. 1979. Pp. viii, 385. \$19.50.

In a brief introduction to his history of the University of Santa Clara, Gerald McKevitt provides a useful summary description of the state of higher education in mid-nineteenth-century America. Pri-

vate sectarian schools predominated, competition for students was intense, institutional mortality rates were high, and quality was low. Furthermore, those colleges that survived into the post-Civil War period were increasingly challenged by the proliferation of state-supported institutions created in response to growing demands for more "democratic" and "practical" higher education.

The University of Santa Clara, founded in 1851 on the ruins of a Franciscan mission, typified the sectarian antebellum college. Organized and operated by Italian Jesuits, the school began as an isolated and strictly-run boarding school for California youth, some of whom enrolled when only six years old. By 1910, however, the features that made Santa Clara a success fifty years earlier seemed likely to consign it to the educational scrap heap. To avoid that, a new president, James P. Morrissey, proceeded to renovate, reconstruct, and reorganize the college. The physical plant was modernized and expanded, the stern student regimen relaxed somewhat, and most important, the academic program significantly overhauled. Besides continuing the traditional Jesuit liberal arts studies, undergraduate training in law, engineering, journalism, and pre-medicine was expanded; students could substitute modern languages and literature for Latin and Greek, and the elective principle was introduced on a limited scale. Finally, the "College of Santa Clara" was officially declared a university at commencement exercises in 1912. Actually, McKevitt notes in his conclusion, Santa Clara did not achieve "full-fledged" university status until well after World War II. Not until then did the introduction of coeducation (1961), graduate programs in the arts and sciences, library facilities, faculty training, and institutional policy bring the university "into line with the prevailing pattern of American Education" (p. 309).

McKevitt's book is gracefully written, thoroughly researched, and considerably more valuable historically than the usual college history. Specifically, it provides an excellent picture of frontier California during the first years of American control and, above all, it offers a splendid and, in this reviewer's opinion, balanced explanation of Jesuit educational ideals. The only mildly distressing features of an otherwise attractively illustrated, well-made, and carefully edited volume are the absence of a bibliography and the placement of notes at the back of the book.

G. M. REED  
*Georgia Institute of Technology*

SUSAN MCINTOSH LLOYD *A Singular School: Abbot Academy, 1828-1973*. Andover, Mass.: Phillips Academy; distributed by University Press of New England, Hanover, N.H. 1979. Pp. x, 626. \$15.00.

Abbot Academy of Andover, Massachusetts, was the younger sister of Phillips Academy, the oldest incorporated boarding school in the United States. Susan McIntosh Lloyd taught history there until 1973 when she and Abbot merged with Phillips.

As intellectual history her study offers a number of revealing anecdotes. One learns that *Butler's Anthology* held sway till the 1890s when it was supplanted by James's *Psychology*. One finds Abbot Principal Philena McKeen relying on Archbishop Ussher's biblical chronology in her church history syllabus of 1879, though in the same year *McGuffey's Reader* dropped the creation story.

As feminist history, the work presents a fascinating assortment of women: alumna Henrietta Jackson Hamlin, wife of the first missionary to Turkey, who withstood hostile officials and rock-throwing neighbors to win the confidence of needy people; drama teacher Emily Hale, confidante, critic, and platonic love of T. S. Eliot; Cathlyn Wilkerson, who as a senior marched against nuclear power in 1962 and as an alumna blew up her parents' Greenwich Village townhouse making bombs for the Weathermen.

As a history of education, *A Singular School* allows one to plot the changing ideals of private female secondary schools. Early Abbot was a homogenous community—evangelical-Protestant in religion and Whig-Republican in politics. It had a cloistered atmosphere, upheld by rules, regulations, and revivals. A barbed-wire fence kept Abbot and Phillips apart. For students Abbot offered a protective environment in which young women could make the transition from childhood to marriage or career. For faculty (mainly unmarried female) it provided identity, influence, and home, though scant monetary reward. Changes were very slow in coming. In 1930 Abbot admitted its first Jewish student; the first blacks appeared in 1953.

But the 1960s wrought a veritable revolution. Dress codes, chapel, discipline, and requirements were swept away by enthusiastic young faculty and new administrators, despite a few raised eyebrows by alumnae and parents. The transformation brought boys and excitement to the campus but also abortion and drugs. When little was left to be liberalized, Abbot took the final step by merging with Phillips Academy. Thus an institution that had withstood one hundred forty-four years of change ceased to be "a singular school." Such was the power of the 1960s.

Lloyd emphasizes Abbot's ties to the Andover community; she is up on the latest feminist studies and is particularly strong on student culture, assuming that the real education went on outside the classroom. But this can be carried too far. Classes and teaching and lessons and learning are slighted to the point where one has difficulty remembering

that this is the history of a school. Though some scholars will applaud this culturalist approach, to this reviewer it often seemed like attending a performance of the play *Hamlet* with the character Hamlet left out.

STEVEN J. NOVAK  
University of California,  
Los Angeles

ALFRED RUNTE. *National Parks: The American Experience*. Lincoln: University of Nebraska Press. 1979. Pp. xiv, 240. \$16.50.

In what is more an essay than a narrative, Alfred Runte offers an interpretive journey through the history of the National Park System. Runte's style is lucid and forceful, at times argumentative, and his choice of themes and manner of presentation make his book an important statement on the history of conservation.

Runte offers a number of theses, many of them unremarkable but previously ignored in scholarly literature on the parks. Unfortunately, he often weakens his arguments with selectivity in supporting data and incomplete research. Runte believes that America's park system owes its origins to national shame over the despoilation of Niagara Falls compounded by a search for identifiable national monuments comparable to the ruins and scenery of Europe. That is a notion others have explored, although never to Runte's depth. He offers a great deal of support for his argument but becomes tendentious when he cites early comparisons of Yellowstone's wonders to Old World castles while ignoring the fact that early visitors more often compared the region to Hades.

More broadly, Runte asserts that national parks have consistently comprised economically "worthless" lands and that "complete conservation" remains a dream. That is so obvious that it is remarkable that it has received little previous scholarly attention. It is, however, a fact of life for the National Park Service and its clients. Finally, one must question Runte's tendency to accuse the nation of shortsightedness in not applying in early years concepts of natural area preservation that are of fairly recent origin.

Despite these and other weaknesses of interpretation, this is a good book. There are, however, a number of major and minor elisions in research and text that prevent its being the comprehensive interpretation that the author seeks. As examples of the minor, the account of the origins of the Antiquities Act glosses over events that spanned many years and wrongly gives sole credit to one man; Runte's search for signs of the "national park idea" ignores the "idea's" invention by Hiram M. Chittenden in



1895; and the account of the See America First campaign ignores the historical context of isolationism and disgust with European wars.

Among major elisions, Runte ignores the majority of the National Park System (its historical area category) to focus solely on the great natural parks. More fundamentally, Runte made no evident use of the wealth of historical work performed within the Park Service, which has for many years explored and analyzed most of the subjects that Runte treats, and which could have supported and refined his case.

DAVID A. CLARY  
*Springville, Indiana*

REID BADGER. *The Great American Fair: The World's Columbian Exposition & American Culture*. Chicago: Nelson Hall. 1979. Pp. xvi, 177. Cloth \$22.95, paper \$10.95.

Nineteenth-century Americans took their world fairs very seriously indeed, says Reid Badger in this interesting study of the Columbian Exposition of 1893 in Chicago, the pre-eminent example of its type. A trip to the fair was worth selling the cooking stove, Hamlin Garland told his parents. Perhaps 5 to 10 percent of Americans, Badger estimates, were drawn to the "White City" in Jackson Park, South Chicago, on the shore of Lake Michigan.

Unlike world fairs after 1920, which were only "remnants of an earlier age" (p. xv), Badger says the nineteenth-century events were microcosms of national culture that provided "an objective focus for cultural self-examination and self expression" (p. xvi). As these quotes and the subtitle of the book imply, the focus of this study, when it moves beyond telling the story of how the fair was originated, planned, financed, and built, is on its general cultural significance. The author's point of view is from the top down, which is reflected, among other places, in the book's numerous illustrations, mainly views of the fair buildings. The fair here is not seen as a social event; relatively little attention is given to the fair itself, most to assembling it and speculating about its symbolic cultural importance.

This vantage point may be justified if America in 1893 still enjoyed, unlike the twentieth century, a more or less homogeneous national culture. But Badger does see a "confusion of symbols" (p. 119) at the fair, between, for example, the rigid classicism of its buildings (formal, academic, monumental) and the "sylvan scenery" (p. 66) of the informal lagoon inspired by Frederick Law Olmsted's twenty-year-old plan for Jackson Park. And he grants that the fair's Midway (from which the modern term originates), whose sideshows were presented under

the guise of authentic foreign and primitive cultures, had an independent vigor.

Was this latter fair, what one might call the people's fair, the same event as the other fair into whose planning and staging this book primarily inquires? What did those millions of visitors who flocked to see the Midway's eleven-ton cheese and Arabian "belly-dances" and who whirled around on the world's first Ferris wheel (since moved to Coney Island) think of the severe Court of Honor that dominated the fair's architecture? Opinions and estimates of the fair's meaning here proceed from the spokesmen of official, usually genteel, culture. It is no wonder that *they* see the fair as a cultural counterweight against social change, as something suitable for "a season of mediocrity and repose" (p. 85), as Henry Watterson said in his dedication oration. What the rest of the people thought, felt, or did is not pursued here.

JOHN TOMSICH  
*Reed College*

ROGER LANE. *Violent Death in the City: Suicide, Accident, and Murder in Nineteenth-Century Philadelphia*. (Commonwealth Fund Books.) Cambridge: Harvard University Press. 1979. Pp. xiii, 193. \$16.50.

In this short book on a big subject, Roger Lane examines violent death in nineteenth-century Philadelphia to learn something about how people lived. He concludes that after about 1870 the homicide rate went down and the suicide rate went up. Accidental deaths also rose, in large measure because of increased dangers of the workplace, mechanical means of transport, and the use of liquid fuels in the home. The kinds of accidental death (such as drowning) associated with impulsive behavior and the miscalculation of risk declined, a finding consistent with lowered homicide rates.

Four of six chapters and two appendixes are devoted to an examination of the numbers, the sources upon which they are based, the methods used to derive them, and assessments of their reliability. Lane has not performed any tests of statistical significance; what he has done is meticulously examine and weigh the evidence to convince the reader that his tables are useful representations of reality. Even the most resolutely nonquantitative historian will be impressed by the care with which Lane has generated his figures and the arguments he offers as to why certain statistics, such as the officially reported number of homicides in the 1840s and 1850s, must be considered systematic undercounts.

More important than any absolute numbers are the rates of change noted above, and more important still, the explanation Lane offers for these



changes. In his view the development of more controlled, depersonalized, and bureaucratic work environments and the public school system designed to produce disciplined workers placed a high premium on predictable, nonimpulsive behavior. People socialized in such school and work environments were more likely to internalize their aggressions and thus be more prone to suicide than homicide. They were also less likely to be involved in drunken horseplay around the docks that helped contribute to the higher rates of drowning before 1870. Men in less-controlled work situations and transients committed more than their share of homicides. Since this group made up a smaller percentage of the total population in 1900 than it had in the middle of the century, the overall homicide rate dropped. As Lane notes, his analysis rests upon a "relatively simple behaviorism" (p. 135), one that might not satisfy a psychologist but does make sense to a historian as consistent with the thrust of social change in the nineteenth century. He also offers a useful explanation of the continued high homicide rates and increasing suicide rates among young black males in recent decades.

The writing is clear, often witty, so that the book is enjoyable as well as useful. It is unfortunate that the notes are grouped at the end, especially since many of them are glosses on the text and not just references.

JAMES F. RICHARDSON  
University of Akron

IAN S. HABERMAN. *The Van Sweringens of Cleveland: The Biography of an Empire*. (Western Reserve Historical Society, number 148.) Cleveland: Western Reserve Historical Society. 1979. Pp. 202. \$10.00.

Seeking to emphasize for a class of second-year MBA students in business history the profound impact of the railroad upon midwestern river towns, I recently posed the question, "What town in Ohio once confidently expected to become one of America's major cities?" From the back benches came the present-minded response, "Cleveland!" Witty, but poor history, for if the railroad blighted Marietta's ambitions, it was a major factor in the development of Cleveland into a leading industrial and commercial center. The city's location on Lake Erie, halfway between Chicago and the northeastern seaboard; the constant improvement of navigation on the Great Lakes, which Cleveland dominated; the phenomenal rise of an American steel industry based on abundant deposits of bituminous coal just to the south of Cleveland and seemingly limitless beds of high-grade iron ore convenient to its port; the emergence of a new basic industry, petroleum,

and a local boy who would organize it into a vigorous enterprise; and, finally, the completion of a heavy-duty, low-cost railroad system that tied it all together efficiently; these had made Cleveland a great modern city by the turn of the century.

These factors and the famous men who were behind them had been at work for three-quarters of a century by the 1920s, when the brothers O. P. and M. J. Van Sweringen leaped into national prominence as assemblers of one of the most complicated railroad empires in history. In his effort to show that the brothers "were the builders of modern Cleveland," therefore, Ian S. Haberman faces an uphill fight, and many readers will conclude that he has not succeeded.

Beginning—and continuing—on a shoestring, the former newsboys started out in real estate. Their great achievement was Shaker Heights, a pioneer among the classic suburban residential developments that were at first the reward and later the refuge of the urban upper middle class. From there "the Vans" moved to an orgy of monumental civic real estate promotion that gave Cleveland its trademark and famous white elephant, Terminal Tower, ten years after the sun had begun to set on the American passenger train.

If the Vans had stopped with their acquisition of the Nickel Plate Railroad and its logical extensions, which the brilliant railroad operating man, John J. Bernet, stood ready to make the efficient heart of a new eastern trunk line, all might have been well. But the Vans slid ever deeper into the quicksands of meaningless railroad empire building. Their empire collapsed, concurrently with railroad profits, after 1930, and the brothers themselves died, less than a year apart, in the midst of costly bankruptcy and fraud proceedings. As usual, only the lawyers and accountants came through smiling.

Even with such dull boys as the Vans, who never played, a much more interesting book might have been written. Haberman has stuck close to published official documents (one, the Senator Burton K. Wheeler committee report, is cited repeatedly), and his book is mostly a summary of complex financial manipulations that are not very interesting in themselves. It is a case of moving from the particular to the particular, against which Allan Nevins warned us. Notably missing is a setting of the American railroad scene at the end of the "Progressive" era, which would have revealed how truly jejune the Vans' financial thrashings and the Inter-state Commerce Commission's bureaucratic posturings were. We do get, in the ersatz entrepreneurship of the Van Sweringens and the dyspeptic utterances of Commissioner Joseph B. Eastman, a strong hint of how far removed from the fray men with truly constructive ideas were. But the real story waiting to be told is John Bernet's remarkable

transformation of the Nickel Plate. After all, the business of business history is business.

ALBRO MARTIN  
Harvard University

DAVID E. NYE. *Henry Ford: "Ignorant Idealist."* (National University Publications Series in American Studies.) Port Washington, N.Y.: Kennikat Press. 1979. Pp. ix, 147. \$12.50.

Henry Ford was a great symbol of the twentieth century, but what, exactly, did he symbolize? Mass production? The five dollar day? The Alger myth? The lost frontier? As with John William Ward's symbolic Jackson, some of the answers point forward toward a brave new tomorrow, and some of them point backward to a nostalgic past. And like the protean Jackson, Henry Ford, as reinterpreted in this thoughtful and incisive study, somehow encompassed them all. He was the perfect bridge between agrarian and industrial America.

In David E. Nye's view, symbols are inherently unstable, compounded of delicate ambiguities and contradictions. Such a symbol was Ford. By embracing both past and future, both individualism and collectivism, both machine and garden, he helped to compose the inevitable tensions of nascent industrialism, reassuring countrymen that the assembly line was in keeping with their vision of an American Arcadia.

In order to explore these tensions, Nye examines both the external and internal Ford. The external Ford—the man the public knew—was a veritable kaleidoscope of images tumbling over one another: the farmer's son, the backyard mechanic, the compassionate titan, the technological seer, the cracker-barrel philosopher, and, finally the embattled tycoon. Ford himself created these motifs, but his public eagerly seized upon them and shaped them to its own felt needs. A few of the motifs had no takers. Ford the dietary enthusiast, Ford the mystic, Ford the believer in precognition and telepathy—these images were decidedly dissonant and for that reason were ignored. Yet it is to these aspects of the Ford personality that we must look, Nye argues, in order to comprehend the real man.

In other words, the real Henry Ford was the internal one. He is to be explained in religious terms, essentially, and the integrating explanation is his belief in reincarnation. That belief enabled Ford to perfect a cosmology that at once accounted for his odd quirks and his true genius. Only by accepting reincarnation and all its implications was Ford able to break through the linearity and sequentality of nineteenth-century time and forge the segmented and accelerated sense of time of the twentieth century.

Nye's book is a valuable contribution to our understanding of Henry Ford the man and the multiple Ford phenomena. It is organized with logic and clarity and enlivened with flashes of insight. Its imperfections—chiefly the title, which even when explained is unfortunate—are relatively minor. It is a solid piece of scholarship.

FRANK W. FOX  
Brigham Young University

DAVID A. WALKER. *Iron Frontier: The Discovery and Early Development of Minnesota's Three Ranges.* (Publications of the Minnesota Historical Society.) St. Paul: The Society. 1979. Pp. xi, 315. \$16.00.

During the last two decades of the nineteenth century the huge, rich iron ore deposits in northeastern Minnesota attracted the attention of American businessmen. Most notable among these were John D. Rockefeller, Andrew Carnegie, John J. Hill, and later the quintessential American capitalist, J. P. Morgan. As usual the major entrepreneurs appeared on the scene after a great deal of the risky exploration and development had already occurred. The accelerated demand for iron ore, rapid technological developments in steel production, and an improved transportation system that rendered the long rail-water shipment of Minnesota iron ores commercially feasible during the last two decades of the nineteenth century, combined with large capital outlays, brought the iron ranges into large-scale production.

The development and exploration of iron ore deposits as large and as remote as those in northeastern Minnesota required capital and organizational ability beyond the capacity of local individuals such as George Stuntz, John Mallmann, Cuyler Adams, or even the Merritt family. David A. Walker argues that the panic of 1893 and the subsequent long depression "served as a turning point in the growth and organization for Minnesota's iron mining industry" (p. 257). Faced with the need for increasingly larger financial commitments, the numerous small operators who had earlier flourished on the iron ranges gave way to a very few large concerns that rapidly gained control of the industry. These large, vertically integrated corporations commanded the necessary capital, the managerial know-how, and the leadership to operate on the scale necessary for profitable exploitation of Minnesota's iron ranges. Walker concludes that "small, independent mining companies, led by enterprising individual pioneer entrepreneurs, could not function in the deteriorating economic situation nor compete in the ruthless world of big business. Unable to match the financial capacities of the

large integrated firms, many were slowly absorbed or squeezed out" (p. 257).

For one who argues in his preface that "men and women create history" and are the "key elements in understanding past events" (p. viii), Walker places great emphasis on the impersonal role of the market, in the form of the panic of 1893, as the decisive factor in the concentration of Minnesota iron mining into a few large corporations.

The role played in the iron ranges by large corporations closely parallels two basic developments in the American economy. First, with the possible exception of farming, the phenomenon Walker analyzes was the common pattern in the frontier extractive industry whether it be fur trading, grazing, lumbering, or mining. In each, a short initial period of exploitation by individual entrepreneurs was rapidly replaced by domination of opportunity by a few large corporate firms. Second, large corporations increasingly dominated in all sectors of the late-nineteenth-century economy. Although the panic of 1893 may have provided the catalyst, developments in the Minnesota iron ranges too closely paralleled the larger experience for it to have been the decisive factor.

While most of my differences with Walker's interpretations are minor, I do think it would have been profitable to have considered the broader implication of the Minnesota frontier iron-mining experience. *Iron Frontier* is well researched, logically organized, and, except for being a bit repetitious in discussing Rockefeller's role, written in a clear, graceful style. It is a valuable contribution to our understanding of the nature of a frontier extractive industry and American business history.

JAMES D. NORRIS  
Northern Illinois University

WILLIAM F. FINE. *Progressive Evolutionism and American Sociology, 1890-1920*. (Studies in American History and Culture, number 8.) Ann Arbor, Mich.: UMI Research Press. 1979. Pp. xv, 302.

In this book William F. Fine tries to do too much. First, he attempts to construct a "paradigm," which he calls "progressive evolutionism," of early American sociological thought. Except for the new terminology, his presentation of the major concepts and assumptions of early sociologists add little if anything to the work of several recent scholars. Furthermore, the paradigm has been presented in a less comprehensible form. Secondly, Fine attempts to explore the internal and external forces that shaped the paradigm. The result is an unsatisfactory mish-mash of elusive abstractions. Apart from the social problems to which the sociologists responded, none of which Fine examines in a careful way, we find

the paradigm is supposed to be a product of, among other things, romanticism, Victorianism, idealism, pragmatism, religion, Matthew Arnold's conception of culture, Darwinism, functional psychology, the German historical school, and Lamarckianism. Finally, the author tries to offer a corrective to what he believes to be the excessive emphasis by recent scholars on professionalization as a key to understanding the shifting orientation of early sociology from an ethical to a scientific perspective. Although Fine does not successfully refute this contention, he does remind us that early sociologists themselves were strongly committed to the methods of science.

This study has been published prematurely. While the author has read widely in both primary and secondary literature of progressive social thought, he should have subjected the results of his study to a rigorous critique by a tough-minded scholar. Then perhaps the diffuse, obtuse, and excessively abstract character of his study could have been remedied.

BENJAMIN G. RADER  
University of Nebraska,  
Lincoln

H. ROGER GRANT. *Insurance Reform: Consumer Action in the Progressive Era*. Ames: Iowa State University Press. 1979. Pp. xi, 202. Cloth \$14.95, paper \$9.50.

Convinced that the effort to reform the insurance industry remains an unexplored part of the record of the Progressive Era, H. Roger Grant investigates the regulatory drives in five key states during the thirty years after 1885. In his exploration of New York, Missouri, Texas, Wisconsin, and Kansas, Grant aligns himself with the consumer-oriented thesis of David P. Thelen and attaches less importance to Robert Wiebe's search for order or Gabriel Kolko's picture of businessmen seeking national legislation for their own purposes. Very much the partisan of policyholders and regulators against insurance companies, the author takes up first the campaign for life insurance reforms, then reviews changes in fire insurance practices, and concludes with chapters on state commissioners and the abortive drive for federal regulation of the industry. Grant has worked hard in primary sources, and he is aware of the relevant scholarly literature. His book remains, however, more a collection of discrete articles than an integrated narrative, and he fails to bring much life or excitement to what is intrinsically an arid subject anyway.

By any standard, the insurance companies deserved much of the criticism they received in the early twentieth century. Internal mismanagement, callous treatment of claims, and corrupt involvement in politics marred their performance.

Thus Grant is properly skeptical of their defenses of their actions. He did not look at the records of the companies themselves, and he often accepts attacks on their conduct as presumptively correct without an attempt to find out what the archives of the industry might disclose. In the case of Thomas W. Lawson and his "Frenzied Finance" muckraking, Grant does not include relevant details about Lawson's career as a stockjobbing promoter. Similarly, the International Policyholder's Committee is described as a "quintessential consumer group" (p. 55) with only the slightest passing recognition of the less worthy motives that Lawson and other sponsors of the IPC brought to their campaign to elect reform directors onto insurance company boards.

Grant is aware that the forces pushing for insurance revision in individual states often owed as much to intra-industry maneuvering as to consumer pressure. The Robertson Insurance Law of 1907 in Texas, requiring the investment of money from Texas in Lone Star State bonds or stocks, was an amalgam of state chauvinism and local insurance company lobbying. But these moments of even-handedness are brief ones, and soon even self-ish actions in Texas and elsewhere receive the author's blessing as consumer benevolences. Indeed, by the book's close, the word "consumer" has lost almost all substantive meaning and has become a kind of synonym for "the people" in their struggles with sinister corporate interests. Whatever their drawbacks as explanations of progressivism, the Wiebe and Kolko arguments at least moved toward a greater analytic complexity in understanding the period. Grant's study suggests that the consumer thesis may be little more than the older, romantic view of progressive reform doing business under a new label.

LEWIS L. GOULD  
University of Texas,  
Austin

WALTER KARP. *The Politics of War: The Story of Two Wars Which Altered Forever the Political Life of the American Republic (1890-1920)*. New York: Harper and Row. 1979. Pp. xiv, 380. \$15.00.

American voters around 1890 awoke from a generation of slumber and reasserted the nation's traditional "republican spirit" as they battled powerful political oligarchies through their new populist and progressive insurgencies. Abandoning the partisan loyalties that had permitted the oligarchies to control voters since the Civil War, Americans challenged political elites and their corporate allies. Losing their control of politics, the oligarchies invented foreign adventures and wars to suffocate their critics in a smokescreen of superficial patriotism. William

McKinley and Woodrow Wilson were the dishonest political geniuses who maneuvered the United States into the wars of 1898 and 1917-18.

So argues Walter Karp in *The Politics of War*, a book that combines remarkable insight and remarkable oversight. Karp writes with refreshing eloquence and passion, and this reviewer believes that a strong case could be made to support his argument, particularly for World War I. Karp plausibly traces for each of the neutrality crises how Wilson felt constrained by public opinion from promoting his desire for American participation. He argues, again plausibly, that neither Wilson nor McKinley was the hesitant champion of intervention that both tried to appear in the months and years before they finally led the country to war.

Unfortunately for those who are inclined to accept Karp's approach, *The Politics of War* is seriously flawed in both conceptualization and research. This book is not based on fresh research in primary sources. Four-fifths of the footnote references in the first third of the book are to secondary sources. Twenty-seven footnotes mention Walter Millis's *Road to War* (1935) and only twenty refer to the *Congressional Record*, Karp's favorite primary source for this section. There are, symbolically, no references to New York newspapers but six to Joseph Wisan's *The Cuban Crisis as Reflected in the New York Press, 1895-1898* (1934). More seriously, Karp seems unaware of major recent interpretations of relationships among voters, populist and progressive insurgencies, and party organizations. Although he discusses relationships between the People's Party and other parties, he seems unaware of Lawrence Goodwyn's *Democratic Promise* (1976) or Peter H. Argersinger's *Populism and Politics* (1975). Claiming to be the first to discover connections between domestic events and foreign policy, he shuns relevant books by Walter LaFeber and William Appleman Williams. He mars many of his shrewdest and most controversial observations—about public opinion and the neutrality debates, for example—by simply failing to give examples or citations.

The conceptual problems are equally severe. If the "oligarchs" favored war to quell insurgencies, why were leading Republican Senate oligarchs like Nelson Aldrich, Orville Platt, John Spooner, and William Allison "antiwar senators," in Karp's phrase (p. 91), who resisted McKinley's drive toward war? Did populists and progressives have foreign policies? Were those policies—or their absence—relevant to McKinley's or Wilson's successes? How did voters try to discipline leaders in foreign policy? Are problems in the American system of representative government, as opposed to the ambitions of particular presidents, relevant to these themes?

Professional historians would err in dismissing



this book because of its obvious shortcomings of scholarship. By reminding us that presidents in 1914–17, as well as 1979–80, have used their powers of foreign policy to distract and intimidate popular opposition to their domestic failures, Karp is addressing issues that go straight to the heart of contemporary as well as past crises in American government.

DAVID P. THELEN  
University of Missouri,  
Columbia

PAUL L. MURPHY. *World War I and the Origin of Civil Liberties in the United States*. (Norton Essays in American History.) New York: W. W. Norton. 1979. Pp. 285. \$16.95.

Paul L. Murphy, to whom both scholars and students are already indebted for his works on the constitutional history of the United States, offers here an extended essay on the era of the First World War, a period that he regards as crucial to American civil liberties. Except in 1798 and again in the Civil War, the protection of civil liberties was of little concern to most Americans. The reason, of course, as Murphy recognizes, was that the federal government during the nineteenth century seldom denied the freedoms of the First Amendment. His point, however, would be clearer if he defined more explicitly the distinction between civil liberties, which protect against government, and civil rights, which depend upon government. Still, more than most authorities, he emphasizes the way in which progressives from Theodore Roosevelt to Woodrow Wilson, in their desire for social control and government regulation, created an environment hostile to traditional individual freedoms. Although he does not accept the contention, which he identifies with the New Left, that "civil liberties were part of the ideology of the middle class, which could be waived at any time" (p. 23), he often follows the legal relativism so popular with many contemporary social scientists. This position may, or may not, be more objective than the old constitutional absolutism. In any case, it deprives his book of the intellectual excitement and moral fervor that characterized Zechariah Chafee's classic study of free speech in the United States.

In his introductory and concluding chapters, Murphy provides a useful overview of the monographic literature, whose help he acknowledges, as well as a brief summary of the post-World War I years when "a good many Americans came to see that domestic radicals and hyphenates were not the threat they had been painted by the Wilson government . . ." (p. 271). It is sad to note, as Murphy does, that with the coming of World War II big

government again took up the crusade against radical dissent. Once this new climate of alleged conspiracy and subversion was imposed, it was easy to revive the old agencies of repression that had been created in World War I.

The main thrust of the book covers this latter and largely familiar story, but Murphy's account is detailed and thorough, especially in its sympathetic summary of the efforts of pacifists and radicals to preserve traditional liberties. Although he is careful to include the war rationale and bureaucratic apologetics offered by Wilson and members of his administration, he makes clear their responsibility and the glaring discrepancy between their liberal rhetoric and reactionary policies. It was the government, not the citizenry, that violated the Constitution and destroyed essential American freedoms. In the forefront of this illiberal repression were the lawyers and judges who, with very few exceptions, failed to stand against the popular hysteria that the president's own demagoguery, as in his Flag Day speech of June 14, 1917, helped to arouse. In contrast to Wilson, the true American patriots were such old-fashioned liberals as Oswald Garrison Villard, editor of the *Nation*, and the radical dissenter Roger Baldwin, principal founder of the American Civil Liberties Union.

ARTHUR A. EKIRCH, JR.  
State University of New York,  
Albany

RUTH WARNER TOWNE. *Senator William J. Stone and the Politics of Compromise*. (National University Publications Series in Political Science.) Port Washington, N.Y.: Kennikat Press. 1979. Pp. x, 278. \$12.50.

Ruth Warner Towne has written a flawed narrative of the public career of William J. Stone, the only Missourian to win election as representative to Congress (1884–1890), as governor (1892–1896), and as United States senator (1902–1918). A native of Kentucky and a Democrat by heritage, Stone moved to Missouri, became a lawyer, and launched his political career at age twenty-four with election as county attorney. In 1914, after war erupted in Europe, Stone became nationally important because he chaired the Senate Committee on Foreign Relations. Towne devotes 40 percent of her book to the period from August 1914 to Stone's death in April 1918.

Towne concludes that Stone was a talented, pragmatic politician who subordinated his own career to advance the welfare of the Democratic Party and the nation. While President Woodrow Wilson "used his magical prose and rational arguments to marshal public and Congressional support," Towne



claims that "Stone moved noiselessly and unobtrusively behind the scenes to smooth the way" (pp. ix-x). She also maintains that during "his long years in office he did not forget the miner in the pit, the immigrant at the gate, the soldier on the way to the front, and the child of the slum" (p. 254).

Unfortunately, Towne's conclusions lack sufficient substantiation. Stone's electoral success attested to obvious ability; his political sacrifices on behalf of party and nation, on the other hand, remain vague. More graphic is Towne's failure to discuss instances when Stone smoothed the way for Wilson's legislative program. She never mentions, for example, The Clayton Anti-Trust Act, The Federal Trade Commission Act, The Adamson Act, The LaFollette Seamen's Act, The Keating-Owen Child Labor Act, and The National Park Service Act. In foreign affairs, Stone broke with Wilson over the president's attempt to arm U.S. merchant ships and five weeks later voted against Wilson's request for a declaration of war. Immediately thereafter, Stone pledged unreserved support for all war measures, although he continued to judge U.S. entry into the war as "the greatest national blunder of history" (p. 233). Wilson, however, needed little help getting war measures through the Senate. Towne also offers no evidence to support her assertion that the senator consistently cared about miners, immigrants, and children of the slums.

Towne's study has other problems. She fails to describe and analyze her statement about "the latent idealism which Stone had carefully suppressed throughout his career" (p. 202); the thrust of her treatment is that he lacked idealism and operated as a politico. At times she draws troublesome conclusions, such as "Progressivism was not so much an ideology as a life style" (p. 71). In her backnotes she cites manuscript collections without providing box or series numbers. In the populism and progressivism portions of her "Essay on Sources" she includes no book published after 1963.

The book does not contribute to a better understanding of progressivism or of the Wilson administration. Beyond presenting factual information about Stone, the study has limitations as biography. The subject merited a more careful and thoughtful treatment.

KEITH W. OLSON  
University of Maryland

LARRY ENGELMANN. *Intemperance: The Lost War Against Liquor*. New York: Free Press. 1979. Pp. xvi, 252. \$10.95.

ERNEST KURTZ. *Not-God: A History of Alcoholics Anonymous*. Center City, Minn.: Hazelden Educational Services. 1979. Pp. xiii, 363. \$12.95.

W. J. RORABAUGH. *The Alcoholic Republic: An American Tradition*. New York: Oxford University Press. 1979. Pp. xvi, 302. \$14.95.

What Larry Engelmann writes about in *Intemperance* is, very narrowly, the lost war in Michigan, especially Detroit, 1920-33. This narrow focus yields a chronicle of choice anecdotes about the Canadian Connection, the Prohibition Navy, rum runners in iceboats sneering at the police, bootleggers in tug boats laying underwater pipelines or towing underwater barges toward insatiable and illicit thirsts on the "dry" side of the river. What it does not yield is any device or dimension through which we can approach these characters or their antagonists in any way that might compel our sympathy or understanding. How are we to understand or to explain what had gone wrong in a society that for so long endured all this trickery, treachery, scheming, running, and shooting? We don't have an answer (even the political history is rather shallow), and, moreover, the solid scholarly work of a dissertation is stripped down to flat journalism: a list of sources, but no footnotes or endnotes, no citations; a narrative padded with a predictable sub-chronicle of national prohibition and studded with journalistic overkill. The saloon, we are told, was "receptacle for rhetorical onanism . . ." a place where a man could "air his feelings about the injustices of American society" (p. 7). For the most part, however, the prose is highly readable and a thoughtful reader can understand why the people of Michigan, who voted for a strong state prohibition law in 1918, were the first to ratify the Twenty-first Amendment.

W. J. Rorabaugh's *The Alcoholic Republic* shows us that, for all the foolishness on the Detroit River, prohibition movements had a lot to prohibit. We see that the per capita consumption of absolute alcohol for the drinking-age (15+) population in 1815 was 7.0 gallons, but in 1850, after Neal Dow's crusades, it was 1.8 gallons. In 1910 it was 2.6 gallons, but after the Dry Decade it was 1.6 gallons in 1940. The emphasis here is on the earlier period, and there are other fascinating facts to consider: for George Washington, the return on an investment of Election Day whiskey in 1758 was little better than two votes per gallon; the affliction called the D.T.s may show more about the culture in which it occurs than about alcohol, and it seems to have become an American affliction in the 1820s when the pattern of drinking comportment shifted from a casual if pervasive nipping toward a more morbid solitary or communal binging; a cup of tea then cost more than a drink of whiskey.

In explaining the "great alcoholic binge of the early nineteenth century" (p. 25), Rorabaugh finds cause in an economy of abundance (it was cheaper to drink whiskey than to transport corn to a mar-

ket), in the American diet (strong drink to wash down all that greasy pork and cornbread), and he finds assistance in some often obvious hypotheses from social science (high goals can be self-defeating, and high aspirations may cause heavy drinking, and so forth). He has looked more carefully at the manuscript sources than anyone before him, and this is lively and often impressive scholarship. But there are problems that cannot be set aside. Regarding per capita consumption, as Rorabaugh himself points out, the more significant questions have to do with who got how much—men more than women, whites more than blacks, westerners more than easterners? Even if we take the guess that maybe 75 percent of the nation's workers hoisted a daily four ounces of booze in 1830—say a little more than two honest drinks a day—this is a shallow base for an “alcoholic republic.” Regarding sheer abundance, the fact of plenty really does not explain excessive drinking, nor does the national diet, however dreadful. Regarding more socially scientific hypotheses, Rorabaugh says that “drinking mores can not be separated from and are functions of ideologies, customs, and social processes” (p. 246), but for all he has shown about drinking, he actually has little to say about the social processes, customs, and ideologies that produced so many drunks.

It may be that the drunks themselves will present some of the understanding we need. Ernest Kurtz's *Not-God* explains what Alcoholics Anonymous can say about American culture. Kurtz makes a careful distinction between the AA program, the history of which flows easily into the scientific study of alcoholism, and the AA fellowship, which has evolved in brilliant turns of pragmatism and doctrine as the founding fathers in the 1930s opened themselves to such complex influences as the Oxford Group, Carl Jung, and William James. The founders were convinced by James and Jung that in his melancholy journey, the alcoholic has actually followed the awesome but illusive promise of transcendence; stumbling down his crooked path, he comes to his ultimate disaster because he has failed to acknowledge his essentially human limitations. After a searching analysis of this axiom, Kurtz suggests that “alcoholism can be a metaphor for the dis-ease of modernity” (p. 255) and thus draws his title—that is, the individual hits bottom because he has denied the “essential not-God-ness of either self or others” (p. 226). Because of this primary insight, he says, the AA fellowship, though rooted in American culture, is able to stand outside it and to define some of its most profound paradoxes. This, Kurtz concludes, is what the drunks have learned the hard way: the need for a philosophy of pluralism, for the shared honesty of mutual vulnerability, for acceptance of an ancient religious principle—that humans

need to give-to and sacrifice-for each other, that strength arises from weakness, that the basis for brotherhood is sharing, but it is the sharing of weakness, not strength. The book is divided into two major parts: “The History” and “The Interpretation.” Both are richly scholarly and lead us carefully into an almost unknown field of American intellectual and social history.

NORMAN H. CLARK  
Everett Community College

VINCENT P. FRANKLIN. *The Education of Black Philadelphia: The Social and Educational History of a Minority Community, 1900-1950*. Philadelphia: University of Pennsylvania Press. 1979. Pp. xxi, 298. \$19.95.

Touted as “the first book-length study of the education of a minority group in the United States,” Vincent P. Franklin's work examines the public school and community-sponsored education of black Philadelphia in the opening half of the twentieth century. “From the perspective of the black community, the major social purpose of public and community educational activities was the advancement of Afro-Americans in the city.”

Separate (black) public schools were a mainstay of Philadelphia's public system of education from 1822 until the mid-1930s when the schools were abolished. (Legislation passed in 1881 made it illegal to force blacks to attend segregated schools, but not illegal to open them and to encourage black parents to send their children to them). White notions of Negro inferiority (conveniently reinforced by the new intelligence testing movement early in the century), combined with the fact that separate schools were the only schools in the system where black teachers were allowed to teach, encouraged the perpetuation of Jim Crow schools. Despite, however, the increase in the number of black public schools, throughout the period the majority of black children were enrolled in racially mixed schools taught by white teachers. The abolition of separate public schools was directly related to the political coming of age of black leaders who succeeded not only in getting themselves elected to office but also in wringing concessions from the political system by effectively ransoming the black vote to whichever party acceded to black demands.

How the black community educated itself is an integral part of the educational history of black Philadelphians. Community educational activities, broadly defined, were sponsored by a network of established and newly created institutions, organizations, and agencies, the most influential of which was the *Philadelphia Tribune*, the city's most important black newspaper. If black social advancement was the ultimate aim of public and community ed-

educational activities, to what extent did they succeed? "Some of the community education programs were effective; others were not." As for the public schools, whose major objective, ostensibly, was to provide the basic tools in order that individuals might compete equally for society's rewards, Franklin found that they provided no such benefit to Philadelphia's black minority. "This investigation found not only that more public schooling did not greatly improve the overall social status of blacks, but . . . that at times the public schools were considered more an obstacle to the achievement of the larger goal of black social advancement."

True to its subtitle, nearly half of this study examines the unsettling impact the Great Migration and the Depression had on black Philadelphians. As presented (and the fault is largely one of organization), the data is only marginally useful for understanding the educational history of black Philadelphia. In other respects as well, the book retains the earmarks of a study begun as a dissertation. By intention the work focuses on the opening five decades of the present century. While Franklin's selection of years might have been questioned had he confined himself to this period, the book, in just over two hundred pages of text, ranges from seventeenth century beginnings to 1979. This chronological overextension requires the author to leave a number of significant themes undeveloped—the important question of ghettoization, to cite but one example. Finally, although only a minor irritant, Franklin occasionally allows his ideological predilections to creep into his narrative. Thus, black Philadelphians of the mid-eighteenth century, most of whom were by then third- and fourth-generation Americans, are referred to as "Africans" or, again, when he describes the black esthetic as unique!

These strictures should not detract from what the author has accomplished. On balance Franklin offers us a thoroughly researched, carefully documented, clearly written, and richly detailed account of the educational history of blacks in the City of Brotherly Love.

GEORGE A. LEVESQUE  
*Indiana State University*

CARLETON MABEE. *Black Education in New York State: From Colonial to Modern Times*. (A New York State Study.) Syracuse, N.Y.: Syracuse University Press. 1979. Pp. xv, 337. \$18.00.

In this study of black education in New York state from the 1780s through the 1930s, Carleton Mabee succeeds admirably in providing "a fresh perspective on one of the most troubled areas of American democracy in our time" (p. xiv). Written with

grace and based upon impressive research, it treats black education within the context of an ever-shifting, but always complex, web of race relations and the changing legal and socioeconomic status of blacks. Among the significant questions discussed in this work are those related to the emergence and development of separate schools for blacks, the degree of control that they exerted over such schools, and the freedom of black educators to engage in protest activities. The treatment of these and other aspects of the black educational experience is perceptive, closely reasoned, and balanced.

Despite the persistence of antiblack prejudice, New York blacks were seldom without white allies in their quest for educational opportunities. Indeed, benevolent whites provided most of the education that blacks received during the four decades following the American Revolution. But black initiative, evident as early as the 1790s, steadily increased and involved nationally known figures such as Charles L. Reason, J. W. C. Pennington, Henry Highland, and Sarah Garnet. Far from being the "obsequious Uncle Toms that later generations imagined them to be" (p. 102), blacks employed a variety of tactics, from self-help and public appeals to protests and boycotts in pursuit of their educational goals. While ministers monopolized leadership on those school issues involving little conflict, Mabee found that proportionally more lower-status blacks led in school activities involving sharp conflict. In the nineteenth century blacks in upstate New York generally enjoyed greater educational advantages than those downstate. The notable exception was Brooklyn, where blacks early succeeded in acquiring a school and in exercising unusual control over it.

Although a law of 1841 presumably opened public schools to all children regardless of race, separate schools for blacks existed well into the twentieth century. But, as Mabee observes, ambivalence characterized the black response to the issue of separatism. While articulate blacks generally desired the right to attend white schools, few were "willing to immolate black teachers on the altar of school integration" (p. 212). The movement against segregated schools that originated upstate reached a climax downstate in the five-year campaign waged by Elizabeth Cisco in Jamaica. Her crusade paved the way for the Roosevelt law of 1900 that, contrary to conventional wisdom, did not entirely eliminate separate black schools. However, school desegregation, once begun downstate, was "carried farther there than upstate" (p. 225). An influx of blacks from the South thwarted the goal of a thoroughly integrated school system just as it appeared within reach. Ironically, southern blacks were attracted to New York "in part because of its relative freedom from the very segregation which their arrival helped to reintroduce" (p. 283). The battle against de facto

school segregation thereafter involved issues, tactics, and false starts reminiscent of an earlier era.

Mabee's volume is educational history at its best and could well serve as a model for future studies of Afro-American education.

WILLARD B. GATEWOOD, JR.  
University of Arkansas,  
Fayetteville

ALBERT CAMARILLO. *Chicanos in a Changing Society: From Mexican Pueblos to American Barrios in Santa Barbara and Southern California, 1848-1930*. Cambridge: Harvard University Press. 1979. Pp. xiii, 326. \$17.50.

Albert Camarillo has written an excellent monograph dealing with the urban Chicano community. He forcefully and convincingly argues that Anglos subordinated Chicanos economically and politically in order to foster the development of California, exploiting minority group members as a pool of cheap labor in times of economic growth and casting them aside in times of recession and depression. People of Mexican descent were a useful adjunct for capitalism only if they were on the lower rungs of the socioeconomic ladder. This monograph provides empirical data to substantiate the thesis advanced by other scholars such as David J. Weber, Leonard Pitt, and Juan Gómez-Quiñones. The author concentrates on Santa Barbara, California, and makes comparative references to other southern California towns. Scholarly examination of this traditionally neglected group fills an important void in the history of the American West since 1848.

In the aftermath of the American military conquest, Mexicanos-Chicanos could not maintain their hegemony for long. Economic depression, fraud, and outright theft caused the loss of most Mexicans' cattle ranches. The Anglo majority, drawn to the area by tourism, agriculture, and related economic developments, gave rise to nativist politicians who stripped Chicanos of their remaining political power through gerrymandering and persecution of political dissidents. This displacement was carried out as part of a campaign to "Americanize" the area. By 1873 Chicanos were limited to unskilled and semiskilled jobs, principally in tourism and agriculture. Residential and social segregation into barrios developed at the same time.

These developments set the stage for the reception of large numbers of Mexican immigrants in the twentieth century. Patterns of discrimination in wages, housing, education, employment, and nativistic outbursts from groups such as the Klu Klux Klan helped to sustain and intensify the earlier nineteenth-century patterns. After 1930 local Anglos employed public agencies to rid the area of many Chicanos and Mexican immigrants.

Throughout his study Camarillo details the internal dynamics of Chicano society in response to change. Within the barrio these "foreigners in their own city" (p. 76) formed mutual aid societies, maintained whatever parts of their culture allowed them by the imperious dictates of law and economics, and sought continuing self-identity through family life. The many efforts at self-help through strikes and political pressure usually resulted in few gains because of the lack of allies, including Anglo-dominated labor unions. There is an admirable analysis of the sometime tension between native californios and newly arrived immigrants.

This is a model study. It is well written, closely argued, and employs a wide range of relevant printed, oral, and manuscript sources. It is a lasting contribution to the new social history in general and Chicano scholarship in particular. One anxiously awaits the author's comparative study of other urban areas. Those interested in ethnic history and western economic development would do well to consult this monograph.

LAWRENCE A. CARDOSO  
University of Wyoming

MYRON BERMAN. *Richmond's Jewry, 1769-1976: Shabbat in Shocktoe*. Charlottesville: University Press of Virginia, for the Jewish Community Federation of Richmond. 1979. Pp. xxii, 438. \$12.50.

MARC LEE RAPHAEL. *Jews and Judaism in a Midwestern Community: Columbus, Ohio, 1840-1975*. Columbus: Ohio Historical Society. 1979. Pp. x, 483. \$19.50.

There was a time, not more than a generation ago when American-Jewish urban history was political-economic and elitist at the best, a local who's who of financial backers at the worst. Since then professional historians have become the writers of most urban histories, and statistical, quantitative methods have been introduced so as to include the 85-90 percent of the population who were not part of the elite. The two books under review present the history of two medium-sized Jewish communities of ten to twenty thousand people, but there their similarities end: Myron Berman's *Richmond's Jewry* is anecdotal and institutional whereas Marc Raphael's study on Columbus, Ohio, is representative of the latest interdisciplinary model, sociological as well as historical.

Berman includes twenty-one illustrations of people, buildings, and so forth in his work but no statistics; Raphael includes ninety-seven statistical tables as well as many photographs and other illustrations. Berman's narrative style, always anecdotal and often disjointed, presents the two-hundred-year



story of Richmond's Jews; the Spanish, German, and East European phase; and ultimately the fusion of all these elements in the modern American Jew, native-born, college-educated, middle-class, proud of his heritage, and aware of his responsibilities for Israel. Assimilation and conversion took their toll especially among the early colonial families, so much so that none of their descendants of today are still Jewish. Even the good Biblical Jewish name Mordecai was denatured into Mor-de-kée (p. 119)!

Edward Nathan Calisch (1865–1946), the twelfth graduate of the recently organized Hebrew Union College, was to serve Richmond's reform temple for fifty-five years. In the mold of classic reform, from which Calisch did not deviate during all these fifty-five years, Americanism and the public image of the Jew became the watchwords of his career. Acceptance of the peoplehood of Israel—as in so many other southern reform congregations—had to await the passing of the old generation and the absorption of the German-Jewish elements by the children of the East-European immigrants. Berman's narrative identifies personalities, institutions, and trends of a southern community; he does not provide real critical insights and clues for perceiving the role played by Jews in the story of the South and in their own symbiosis with modern America.

Raphael succeeds in these respects in his analysis of Columbus, Ohio, and our historical and sociological understanding is carried a step further. The study reflects the spirit of the "new history" interested in the lives of ordinary people as much as that of the elite, in wide-ranging analogies and comparisons rather than often only trivial episodes. (Richmond's mayor assured a certain family that a road commemorating their grandmother would not be renamed [Berman, p. 267]; Raphael lists 1932–33 sermon titles of two rabbis in tables 48 and 49.)

Sociological, economic, political, and demographic data drawn from state and federal census schedules, tax lists, school and court records, business and city directories as well as newspapers, synagogue, and federation files have become the tools for quantifiable analysis and extensive comparisons with data from other communities. Much of this raw material is presented in the almost one hundred statistical tables by Raphael and his research team; this reviewer regrets, however, that too often no explanations for these mathematical constructions are provided (for example, what is "N" in Tables 86 and 87?). Table 29 enumerates the Fulton Street School First Graders in 1911–12, but why are the other grades omitted? In spite of these minor criticisms *Jews and Judaism in a Midwestern Community* constitutes a major breakthrough in the writing of Jewish local history in America.

FRANK ROSENTHAL  
East Los Angeles College

JEFFREY S. GUROCK. *When Harlem Was Jewish, 1870–1930*. New York: Columbia University Press. 1979. Pp. xi, 216. \$15.00.

Before its renown as the black metropolis, Harlem, as a remote district of New York City, was an ethnically diverse area inhabited by Irish, Germans, and Jews mainly of German origin. The extension of mass transit lines to Harlem from the 1890s stimulated building activity to a feverish although irregular pace, and these processes in their turn made for an area densely settled by Italians, some blacks, and especially by Russian Jewish immigrants, most of whom had recently moved there from the Lower East Side. In 1900 Harlem was already the home of some 17,000 Russian Jews, and their number boomed to 178,000 in 1918 and declined somewhat to 168,000 in 1923 before it dropped to near the vanishing point in 1930 when no more than 5,000 remained.

Jeffrey S. Gurock's excellent study distinguishes two segments within Harlem Jewry. One consisted of newly middle-class East European Jews as well as the older German stock, who were merchants, professionals, and white collar workers. The other segment was composed of immigrants who had left the Lower East Side because they could find slightly better housing and employment in the uptown neighborhood. They worked at crafts like carpentry, painting, and tobacco manufacture, as well as petty trade. (Garment workers, who worked long hours in an industry concentrated far downtown, rarely were able to live so far from their work as Harlem.) Gurock argues that the establishment of this "Second Ghetto" was less a matter of upward mobility than the outcome of extensive demolition on the Lower East Side, which forced thousands of residents to move away. Their area was a stronghold of socialism, trade unionism, fraternal societies, and little orthodox synagogues. There were few large, but many small, strikes and a great deal of militant consumerism aimed against offending butchers, bakers, and landlords.

Gurock regards the "Second Ghetto" as merely transitory, for the future lay with the prospering, upwardly mobile Jews of Central Harlem. Here, the new and the old Jewish stock accepted each other and cooperated willingly for communal purposes. They established modern Jewish schools, community centers, and institutional synagogues, all directed to harmonizing Jewish tradition with American life.

If their efforts mark the beginning of American Jewry's evolution into its contemporary forms, as Gurock argues, he should have examined those institutions somewhat more closely. One would also wish for greater attention to the inter-ethnic relations and for fuller exposition of the swift decline of



Harlem Jewry. It seems exaggerated to say that the World War I housing shortage that ruptured the "residential equilibrium" within Harlem brought about this decline. Did mass black settlement force out Jews and other whites, or did blacks enter en masse behind departing Jews and other whites? One would welcome a more definitive statement. Regrettably, no comparison is made with other cities, (except Chicago—the sociologists' model) or even with other areas of New York City.

Gurock's work is thorough and exact, its many details always serving the larger picture he draws. He employs press sources, census schedules, real estate literature, and local ephemera. Although his maps are too schematic to be useful, the index, typography, and photographs are good. Gurock's literary style, while readable, oscillates between the clear and the ponderous. But these are minor qualifications concerning a book that is a significant, admirable contribution to New York City and American Jewish history.

LLOYD P. GARTNER  
Tel-Aviv University

DAVID D. LEE. *Tennessee in Turmoil: Politics in the Volunteer State, 1920-1932*. Memphis, Tenn.: Memphis State University Press. 1979. Pp. xvi, 204. \$15.00.

This brief but informative book details the changes that took place in Tennessee politics between 1920 and 1932. David D. Lee demonstrates that the election of Austin Peay to the governorship in 1923 set in motion a shift of power from the legislative to the executive branch of state government. Peay's increased control of expenditures produced a much-needed program of improvements in education, roads, and parks and a modernized and more equitable system of taxation.

Although Peay was a strong leader who kept corruption to a minimum, after his death in 1927 the newly centralized government of Tennessee fell into the hands of a political machine dominated by publisher Luke Lea and businessman Rogers Caldwell. The machine's control seemed absolute until 1931 when its leaders were accused of misusing state funds in an attempt to bail out Caldwell's failing business ventures. The collapse of the Lea-Caldwell faction coincided with the ascendance of a more durable political organization, that of Edward H. Crump of Memphis. Peay's efforts to centralize and improve state government had actually created the foundation for machine rule in Tennessee.

Lee's summary is valuable, but his conclusions raise some questions about the assumptions underlying his research. He characterizes the 1920s as a period when the "sibling processes of urbanization and industrialization were gradually creating a new

power base at the expense of the farmers and rural-oriented small town merchants and professionals" (p. 3). Thus, he describes Austin Peay's "business progressive" program of modernized government and expanded public services and facilities as one designed to "meet the needs of an increasingly industrial, urban-oriented society" (pp. xii-iv). Lee's interpretations seem logical enough until he demonstrates that Peay's policies were "demanded, supported and enacted largely by the people from the farms and small towns" while "major urban areas provided the most determined opposition" (p. 152). Lee surmises that urban voters objected to being taxed for better roads and schools in the countryside but makes no effort to explain why a program he describes as urban-oriented actually appeared to be of greater benefit to rural areas.

Lee's apparently contradictory interpretations may be related to the assumption that urbanization and industrialization enjoyed a "sibling" relationship in Tennessee. As of 1930 the rural South was still nearly three times as industrialized as rural portions of the manufacturing belt. Lee chronicles the growth of industry in major cities but offers no evidence that Tennessee's overall pattern of industrialization was more urbanized than that of the South as a whole. In fact, though it remained sizable, the percentage of the state's manufacturing employees residing in large urban counties decreased slightly between 1919 and 1929. Lee assumes that farmers and small-town merchants were the backbone of Peay's support, but a program that promised improved transportation and better-educated workers largely at the expense of big-city taxpayers must also have been appealing to industrial interests based in the state's rural and small urban counties.

Closer attention to the actual distribution of industrial development in Tennessee might have enabled Lee to present a more plausible interpretation of the events he describes. Similar attention to the locational patterns of economic development in other states might assist historians in understanding not only business progressivism but also the persistent rural influences that shaped the South's politics and culture long after its economy became less dependent on its farms than its factories.

JAMES C. COBB  
University of Northern Iowa

GARY B. OSTROWER. *Collective Insecurity: The United States and the League of Nations during the Early Thirties*. Lewisburg, Pa.: Bucknell University Press. 1979. Pp. 287. \$17.50.

Despite a title that promises a good deal more, this slender monograph deals essentially with the relationship of the United States to the League of Na-

tions during the Manchurian crisis of the early thirties, to which all but one of its chapters is devoted. It thus traverses ground already well covered in Christopher Thorne's *The Limits of Foreign Policy: The West, the League and the Far Eastern Crisis of 1931-1933* (1972) and Armin Rappaport's *Henry L. Stimson and Japan, 1931-1933* (1963). The final chapter touches on the American decision to join the International Labor Organization in 1934 and on the unsuccessful attempt to join the World Court in 1935.

The juxtaposition of these events is intended to demonstrate the existence of "an internationalist impulse . . . within the Department of State and among an important segment of the politically articulate public" (p. 199) but does not do so convincingly. Even Stimson, as Rappaport had already shown and as Gary B. Ostrower now confirms, was "internationalist" only sporadically, and his feints at cooperation with the League fell far short of anything that might indicate a commitment to collective security. Cited signs and portents, such as the efforts in 1933 to provide for regular contacts with the League through reorganization both in Washington and Geneva, were largely confined to internal memoranda that produced no actual results.

The evidence here and elsewhere indicates, in fact, that the state department was divided on most relevant issues and that so-called internationalism reared its head only fitfully in the statements and actions of Stimson, Hornbeck, Castle, and other major protagonists. It does little to substantiate the claim, made at least for the late twenties, that "(a)n isolationist public partially handcuffed an internationalist State Department" (p. 55), a claim that Ostrower himself ultimately dilutes to the vanishing point by noting that within the department were "strong countercurrents that, at certain crucial points, checked both internationalism and the evolution toward a collective security system" (p. 199) and that "(t)he issue of public opinion is likewise less clear than it has often seemed" (p. 202). Indeed, the most that could possibly be claimed is that the state department demonstrated a streak of activism that at times resulted in a modicum of international cooperation. In relation to the League, Stimson and the others played it safe, "so safe, in fact," as Ostrower himself points out on one occasion, "that many League members were unable to determine where America actually stood" (p. 152).

The book is not helped by the author's penchant for clichés and for meaningless words and phrases. What is "a willingness to semicooperate" (p. 53), "the most humane and non-Tory of Conservatives" (p. 71), or "relatively small changes made outside the public eye" (p. 167)? How does one act in "a bold yet cautious manner" (p. 102)? And what do we know after we are told that "all pleas fell on deaf

ears. Stimson therefore ate his cake and had it too" (p. 100)?

Ostrower has clearly done his homework and mined the relevant sources, often to good effect. But his central proposition is essentially a weak one, and his style does nothing to disguise that.

MANFRED JONAS  
Union College

IRWIN F. GELLMAN. *Good Neighbor Diplomacy: United States Policies in Latin America, 1933-1945*. (John Hopkins University Studies in Historical and Political Science, 97th series, number 2.) Baltimore: John Hopkins University Press. 1979. Pp. xii, 296. \$18.50.

On the threshold of the 1980s a major university press has chosen to publish a work that ignores critical scholarship of the last two decades. Suddenly issues so painfully raised by William Appleman Williams, Lloyd Gardner, Robert F. Smith, James Petras, Andre Gunder Frank, and others no longer exist. By a miracle of evasion the reader is thrust back into the era of Fourth of July history (New Deal version). The Roosevelt presidency, Irwin F. Gellman solemnly writes, was "the golden age of Pan American cooperation" (p. 227). Roosevelt himself was not merely president of the United States but a "citizen of the Americas" (p. 209). Gellman even revives the inaccurate cliché that subsequent presidents have "neglected the Western Hemisphere, with tragic consequences" (p. 228). To be sure, he makes no effort to substantiate the latter charge. His story ends within three months after Roosevelt's death. He then merely lists resignation dates of "leading" Roosevelt-era policy makers, as if a turnover of personnel proves discontinuity of policy.

The problem is not that Gellman has failed to do his homework, for the book contains a mass of data. The difficulty lies in the way he organizes and interprets his material. What is one to make of an analysis dismissing the Inter-American Bank in two paragraphs (p. 160) but devoting two pages to Sumner Welles's homosexuality (pp. 177-78)? Gellman seems unable to assess the relative importance of, or indeed the relationship between, personal and structural factors. His characters live in a world unconstrained by socioeconomic structure or internalized political assumptions, limited only by that phenomenon described as "the depression" and a troublesome legacy of "interventionism." This endows them with both more and less freedom of action than they actually had. For a far more sophisticated approach, the reader should consult Lloyd Gardner's *Economic Aspects of New Deal Diplomacy* (1964), a work mentioned briefly in Gellman's footnotes but the import of which seems to have escaped him.

Gellman's preoccupation with personalities leads him in some odd directions. On one hand, he offers a gratuitous lampoon of Hull's speech impediment; on the other, his uncritical acceptance of testimony contained in interviews granted him by the late Nelson Rockefeller blinds him to Rockefeller's own role in ensuring policy continuity after Roosevelt's death. But then, that continuity Gellman cannot afford to recognize anyway since it wreaks havoc with his thesis.

In his summary of Roosevelt's contribution to the Good Neighbor Policy, Gellman observes that the president's economic understanding was "limited" (p. 227)—an ironic accusation considering Gellman's own failure to integrate economic issues. Not once in his discussion of the fall of Grau San Martín and the subsequent abrogation of the Platt Amendment in Cuba does he even mention sugar (pp. 36–37). He recognizes the "political" nature of wartime loans to Latin America, but never relates these loans to postwar planning or to issues of permanent peacetime economic strategy (p. 162). He entirely neglects U.S. efforts to exercise veto power over Latin American national development corporations and simply dismisses crucial economic negotiations at the 1945 Chapultepec Conference as "disappointing" (p. 207). Beyond this, Gellman's insistence upon personalities leads to a sharp oversimplification of Latin American history. In discussing 1944 state department policy toward Argentina, he writes: "Hull never realized that Perón thrived and rose to power because of the secretary's keeping Argentina in a state of flux through his inconsistent actions" (p. 195). Argentine historians will laugh this interpretation out of court. The 1944 Argentine political situation was infinitely more complex than this simplistic approach can allow.

Finally, Gellman's book begins and ends with an inaccurate and pompous assertion that other writers have "virtually ignored" his subject and that he is therefore breaking new ground. Other writers have not ignored the Good Neighbor Policy; Gellman has merely found it convenient, indeed necessary, to ignore them. Were this book to be taken seriously, it would set back the cause of scholarship by at least twenty years.

DAVID GREEN  
University of Saskatchewan

MILLARD L. GIESKE. *Minnesota Farmer-Laborism: The Third-Party Alternative*. Minneapolis: University of Minnesota Press. 1979. Pp. ix, 389. \$15.00.

While the presence of a workable third party is unusual in American politics, it was not totally unexpected in Minnesota when the Farmer-Labor Party

emerged. Aiding its development was a strong tradition of agrarian discontent, progressivism, and trade unionism. In detail Millard L. Gieske chronicles the successes and failures of the party from its inception in 1918 until its final merger with the Democratic Party in 1944. In the process, Farmer-Laborites won state and national offices and replaced Democrats as the principal opposition to Republicans. But the third party experience was troubled with constant internal schism and failed in becoming a national party.

In this solid study, based on thorough research in manuscript collections and newspapers, Gieske emphasizes the origins and structure of the party, electoral patterns, ideological differences, and personalities in the movement. Initial F-L success came in the 1920s with the election of Senators Henrik Shipstead and Magnus Johnson. These victories were in the "moderate" and "farmer" wing of the party, as exemplified by Shipstead, an independent politician who "deemphasized partisanship and collectivist ideology" (p. 139) and later became a Republican. In Gieske's assessment, support of public ownership distinguished "left" from "moderate" in the party (both groups left of most Republicans and Democrats).

F-L Party appeal peaked during the depression years when Governor Floyd B. Olson became the movement's unquestioned leader. While Olson espoused certain radical views, Gieske argues convincingly that he was mainly a "pragmatic liberal" (p. 145) who endorsed the New Deal and downplayed the leftist 1934 F-L platform. But F-L patronage problems, Olson's death in 1936, charges of Communism and anti-Semitism, foreign policy issues, and the appeal of liberal Republican Harold Stassen spelled defeat for the party. The 1938 election, including the bitter Elmer Benson-Hjalmar Petersen primary fight, signaled the decline.

Gieske's study is informative on Minnesota's Democratic Party weakness and gradual revival during the era, with periodic overtures for DFL merger, consistent Irish-American candidates, and an intraparty split. Discussion of complex internal F-L organizations (for example, the F-L Association) is helpful. Election tables are good.

The book does have shortcomings. There is some repetition, a pre-occupation with labeling every individual "left" or "moderate-conservative," and the narrative style is a bit heavy. Ethnicity, particularly Scandinavian appeal and the dominance of Scandinavian-American candidates, and F-L presence on the Iron Range and in Duluth could have been discussed in greater depth. Oral history might have been expanded and publications by James Shields, James Weinstein, and James Youngdale noted. Charles Lindbergh, Sr., did not support Johnson in 1923 (p. 78).

On balance, *Minnesota Farmer-Laborism* is a significant contribution in understanding F-L history and becomes a principal source on the party. In Gieske's judgment, the movement's major contribution was not its third-partyism or economic theories, but "the tradition of effective leadership" (p. 172).

BRUCE L. LARSON  
Mankato State University

DONALD L. MILLER. *The New American Radicalism: Alfred M. Bingham and Non-Marxian Insurgency in the New Deal Era*. (National University Publications Series in Political Science.) Port Washington, N.Y.: Kennikat Press. 1979. Pp. viii, 240. \$17.50.

Donald L. Miller's account of the life and times of Alfred Bingham advances the lesson Frank Warren drew almost fifteen years ago in his examination of Bingham's independent reform thought: "If historians are ever to reach the core of liberalism—1930's variety—it is necessary that they become more than peripherally interested in the liberals whose commitment was to principles rather than to an administration." Noting that efforts to discern principled liberalism outside the New Deal have fixed on general ideas, rather than the lives and actions of particular reformers, Miller seeks to add insights that can come only from close examination of an activist. His story unfolds as a morality tale about a Connecticut Yankee aristocrat, turned Galahad, who never finds the Holy Grail of "New American Radicalism" and must then devise a measure of consolation and new purpose.

Bingham was most provocative in his emphasis on the middle class as the locus of a "New Radicalism." He insisted that America was an "exceptional" case where there was no fixed lower class to revolutionize, only an aggregate of have-nots striving to achieve the middle-class stature that they believed was their proper lot. Those who did have middle-class status had greater revolutionary potential, for they were losing their security and were ripe for outrage at the ever-dwindling elite that was coming to own most of the property. This conception owed much to Veblen's theory that status-seeking was more important than class loyalty and to Berle's and Means's findings in *The Modern Corporation* (1932) of the growing concentration of wealth.

Had Bingham aligned himself in the beginning with the New Deal, he might have gained some influence and authority—even a niche in government. Such an establishment stance would have set him up as a critic who presaged the postwar revival of Tocqueville's view of American middle-road consensus. Instead, Bingham chose radical opposition to the New Deal and exposed himself to deadly fire from all sides. Yet, as Miller astutely describes,

Bingham's futile efforts to promote a New Radicalism based on middle-class support for a socialized society helped reveal the inherent nature of many of the more influential movers and shakers who opposed him. By seeking a coalition with the left, for example, Bingham's farm-labor group moved the Marxian socialists to show that they would not cooperate when they could not control. On the other side, Bingham's carefully detailed case for middle-class democratic planning rebutted proclaimers of an elitist "managerial revolution," like James Burnham, and so provided continuity between early hopeful visions of an economy of shared abundance and present-day movement, under the press of scarcity, toward some sort of equitable planning. And, finally, Miller's focus on Bingham amidst Deweyan hopefulness helps explain how the triumph by Lewis Mumford, Reinhold Niebuhr, and other "realists" over "naïve" faith in essential goodness was Pyrrhic. It led, not to the expected era of greater humane and esthetic sensibility but to existential gloom in the absence of anything as constructive as the old pragmatic zest for social experimentation.

Because he remained true to his ideals, Bingham faced the coming of war and the end of reformism with a regretful sense of failure. And yet, as Miller shows with cogent analysis, Bingham's ideas have retained value by providing unique perspective on problems in American life that have never been resolved.

ALAN LAWSON  
Boston College

JOHN F. STACK, JR. *International Conflict in an American City: Boston's Irish, Italians, and Jews, 1935-1944*. (Contributions in Political Science, number 26.) Westport, Conn.: Greenwood Press. 1979. Pp. xiv, 181. \$22.50.

John F. Stack's laws governing ethnic conflict stress the battleground of rhetoric, the importance of isolated ethnic enclaves or absence of residential contiguity, and the value of international issues that foster group consensus against a common foe. He uses these criteria to explain the interethnic antagonisms among Irish, Italian, and Jewish Bostonians during the decade 1935-1944. He collects evidence from newspapers such as *Gazzetta del Massachusetts*, the *Boston Pilot* (Roman Catholic archdiocesan publication), and the *Jewish Advocate*. He relies heavily on the dissertation of Charles Trout, "Boston During the Great Depression 1928-1940" (1972) and John Diggins's *Mussolini and Fascism, the View from America* (1972).

In describing the support of Jewish liberals for Roosevelt's domestic program, he ignores their criti-



cism of Roosevelt's policy on Jewish refugees. Stack credits Father Coughlin's antisemitic, anticomunistic, and profascist Spain policies with the coalescing of Irish opinion. But he mentions, only in passing, the socioeconomic factors of the Irish Bostonian working class. And he does not consider the possible appeal of Coughlin's policies to the profascist Italian Bostonian working class. Stack also assumes monolithic support for Mussolini among Boston's Italians. He fails to examine the role of internal conflict among Italians spearheaded by labor leaders Luigi Antonini of the ILG, Frank Bellanca of the ACWU, and antifascist Italian exile Gaetano Salvemini, who spent these years teaching history at Harvard.

Stack accepts uncritically the autobiographical, intuitive study of Italian Americans by Richard Gambino and the clever asides of Andrew Greeley describing Irish-American behavior as documentary evidence. Throughout the book he mentions ethnic conflict between Boston's Italians and blacks, Irish and Jews, Italians and Jews, but he provides no specific, detailed examples. Stack's attempt at a complex analysis of ethnic group behavior responding to international issues while beset with the problems of depression, local politics, and aspirations of mobility becomes entangled in rhetoric and never gets beyond its veneer to a more critical interpretation of ethnic popular opinion and behavior.

JEAN SCARPACI  
Towson State University

STUART E. KNEE. *The Concept of Zionist Dissent in the American Mind, 1917-1941*. New York: Robert Speller and Sons. 1979. Pp. xiii, 268. \$12.95.

Today's overwhelmingly pro-Israel sentiment within the American Jewish community often obscures the fact that Jewish nationalism was a minority viewpoint prior to the Holocaust. While many American Jews casually sport buttons or bumper stickers proclaiming "I Am A Zionist," even today less than one-sixth of American Jewry belongs to organized Zionist bodies, and the number was far smaller in the days before World War II. Thus a study of anti-Zionist and non-Zionist attitudes in the United States would be a welcome addition to the growing body of literature on American Jewry and the development of a pro-Zionist consensus.

Regrettably, Stuart E. Knee's book fails to fill this gap. While it purports to deal with opposition to Zionism, the reader may have great difficulty in discerning the precise meaning of his terms. For undisclosed reasons, Knee seems to have adopted a Weizmann-Labor Zionist program as the normative form of the movement, although Weizmann and the Labor factions were not that closely

aligned, and Labor Zionism never commanded more than a small minority among Zionists in this country. Against this supposed norm, though, Knee asserts that the Brandeis-Mack group, advocating an Americanized version of the movement, should be seen as similar to antinationalist Jews such as the assimilationists in the Reform movement or Protestant sects with ties to missions in the Middle East.

The author has gathered a great number of quotations but fails to impose an orderly analysis on his material. He apparently takes his evidence uncritically and at face value, without placing it in context. Moreover, disturbing factual errors undermine the reliability of the work. He suggests, for example, that "less than ten percent" of America's 3.3 million Jews were enrolled in Zionist organizations in 1914; in fact, that figure should be one-third of one percent, and even in 1919, at the height of the Brandeis group's power, only some 6 percent paid the shekel. He declares that Weizmann snubbed the Americans by not inviting their participation in the Palestine Commission in 1918 (p. 124). Just the opposite is true; Weizmann begged Brandeis to join the Commission or at the very least to designate some Americans to go with him, but Brandeis steadfastly refused on the advice of the state department. Similarly, Knee's discussion of Reform attitudes would have benefited enormously had he seen David Polish's fine book, *Remember the Days*.

There is much material in this book, but scholars wishing to use it must do so carefully and selectively. We still await a study of anti- and non-Zionist attitudes in this country.

MELVIN I. UROFSKY  
Virginia Commonwealth University

ZVI GANIN. *Truman, American Jewry, and Israel, 1945-1948*. New York: Holmes and Meier. 1979. Pp. xvi, 238. Cloth \$24.50, paper \$12.50.

Scholars of American-Middle East relations will be both pleased and disappointed by various aspects of this book. On the positive side of the ledger, Zvi Ganin's use of material from previously unavailable sources enhances his work significantly. Particularly commendable is his ability to draw relevant information from the Foreign Relations of the United States series as well as state department records relating to Palestine.

In large measure because of Ganin's reliance upon these recently released primary sources, this volume represents the clearest and best-documented account of the state department's effort to block the establishment of a Jewish state in Palestine. Another strength of the book is its thoroughly researched and thoughtful account of the American Zionist organizations' lobbying role to promote sup-



port for a Jewish state on the part of the United States government. A major thesis of the book is the author's contention that the Zionist organizations in this country have been given far too much credit for bringing the American government around to a position of support for Zionist goals.

One major figure in American Zionism, Rabbi Abba Hillel Silver, comes under particular scrutiny. Cochairman of the American Zionist Emergency Committee (AZEC), Silver is generally acknowledged to have been a crucial figure in making American Jewry a potent political force. Ganin demonstrates, however, that Silver so alienated President Harry Truman on a personal level that he diminished not only his own effectiveness but also that of AZEC.

Ganin, who teaches at Beit Berl College in Israel, is less successful in analyzing Truman and his pattern of careening back and forth between support and nonsupport for the Zionist cause from 1945 to 1948. Although the author ascribes humanitarian reasons to Truman's ultimate commitment to Zionist aims, he is totally unable to substantiate his claim that it was Truman's concern for displaced Jews, and not political considerations, that was the motivating factor in Truman finally supporting pro-Zionist positions.

Truman's dramatic recognition of the state of Israel only eleven minutes after it came into existence occurred during a critical moment in the president's uphill election campaign of 1948. Ironically, instead of dispelling the thesis that Truman operated largely within a political context when he supported Israel, the evidence presented by Ganin strongly reinforces such an analysis.

The author displays little understanding of the American political system, especially how the system can function during a presidential election year. Truman was influenced, writes Ganin, "by the strong support . . . large segments of public opinion had given to the Zionist cause" (p. xv). Actually, polls indicated that the Palestine issue was not one in which the general public took much interest. Yet Ganin misses the key point that within the presidential electoral system small minorities—Jews in this case—located in closely contested states with large electoral votes, can easily swing an election. Since at that time no significant opposition to a pro-Zionist policy existed in American politics, the president had nothing to lose politically, and possible victory to gain, by supporting the demands of American Jewish voters.

A serious flaw is the book's partisan slant. Ganin is long on understanding Zionism's supporters and short on understanding its opponents. Typical of Ganin's slant is his remarkable understatement when he refers to how the Arabs viewed the creation of Israel. A Jewish state in Palestine, Ganin

writes, "seemed to involve an injustice to the Arabs" (p. xiv) [emphasis mine]. King Abdul Aziz Ibn Saud, ruler of Saudi Arabia, is described as a "fanatic Moslem" (p. 111); no one is referred to as a "fanatic Zionist" or a "fanatic Jew." Similar examples abound.

In some areas the author simply does not have the evidence to back up his claims. For example, Ganin challenges the traditional interpretation of the role Arabs had in trying to influence Truman when he says that the president "was subjected to steady, strong, and effective Arab pressures" (p. 110). The problem in this instance is that Ganin fails to present even the slightest evidence that would prove this controversial point.

JOHN SNETSINGER

California Polytechnic State University,  
San Luis Obispo

RICHARD S. TEDLOW. *Keeping the Corporate Image: Public Relations and Business, 1900-1950*. (Industrial Development and the Social Fabric, number 3.) Greenwich, Conn.: JAI Press. 1979. Pp. xx, 233. \$26.50.

Richard S. Tedlow has written the best and most complete history of public relations, the vocation and the corporate policy. With wider chronological scope, greater concern for the political environment of business, and extensive use of recent secondary works, his book surpasses and supplants the reviewer's own earlier work. As a useful addition to American business history, *Keeping the Corporate Image* should be the first reference for those interested in the history of PR.

Despite obvious merits, the book offers surprisingly little new interpretive insight, however. Tedlow follows earlier explanations for the emergence and growth of corporate PR as both a defensive reaction against political threats and labor unions and an institutional search for order, rationalization, and efficient organization in the era of bigness and impersonalization. Not an apologist for corporate policies or the PR specialists, Tedlow joins scholars who emphasize the diversity and transformation of business opinions and practices, and he criticizes radical scholars who impose a consensus where none existed. His own stand toward PR he calls ambiguous.

Examining the half century of the vocation, Tedlow correctly points out its limitations and pretensions. PR specialists exaggerated their manipulative power over public opinion and their influence within corporate policy-making, the "two-way street." Most PR specialists identified too closely with their employers' interests to serve as impartial advocates of substantive reforms, and those few who tried usually failed. PR acquired, according to Ted-

low, only an "essentially peripheral influence" (p. 201) within corporate policy-making, mainly concerned with the consciousness and style of publicity. Nevertheless, he is unwilling to discount entirely the vocation's so-called reforming influence on business and suggests that, in any case, it probably did contribute to the American public's acceptance of the corporate system in the twentieth century.

At just that point, when he moves beyond the history of the vocation, his book is more ambitious, and less satisfactory. The main problem is not lack of concern for context but improper perspective and proportion. While admitting that PR's influence was peripheral, Tedlow examines the transformation of business opinions and practices as expressions of what he calls public relations-mindedness. Thus he explores activities of such business organizations as the NAM and the CED, but on those subjects the book is too derivative and superficial. (His account of the CED's position on the Full Employment Bill, for example, ignores the evidence of Stephen Bailey's standard work, *Congress Makes a Law*.) More importantly, focus on a nebulous public relations mindedness just will not illuminate the transformation of basic business policies regarding prices, wages, unions, quality of products and services, consolidation, and competition. Conversely, without proper emphasis on those basic issues, the reputation of business and the effectiveness of publicity cannot be adequately explained.

Whatever the book's limitations, it certainly deserves better from the publisher than numerous typographical errors, inadequate footnotes, and an unhelpful index.

ALAN RAUCHER  
Wayne State University

WARREN JAMES BELASCO. *Americans on the Road: From Autocamp to Motel, 1910-1945*. Cambridge, Mass.: MIT Press. 1979. Pp. ix, 212. \$14.95.

Seldom does one confront a book, not to say an excellent book, in which the author begins with expressions of dissatisfaction with his chosen point of departure. "Sharing recent fears that cultural history may have relied too heavily on purely literary and intellectual concerns," Warren James Belasco writes near the end of his preface, "I saw the motel as a revealing interface between business and culture, a material embodiment of tourist dreams of community, independence, equality and spontaneity. . . . [Yet] like the car, the motel did not fulfill the dreams that created it. Thus my journey ended with a feeling of frustration, a sense that for all this movement basic needs remained unsatisfied. Maybe motorists took the wrong path."

Belasco, at any rate, after dallying with numerous points of departure, will be found to have taken the right path. His focus, which grows stronger as his narrative develops, is on what large numbers of Americans actually did *with* their cars, rather than what cars did to America.

"Gypsying" is what Belasco calls the first stage, the "squatter anarchist" stage, of the automobile vacation on the road. One of the thoughtless characteristics of this vacation mode, one that grew ever more common and ever more objectionable to residents along the way, was the gypsies' practice of abandoning garbage and other litter as they drove off. This abuse spread as "gypsying" grew with the growth of the automobile business and ultimately called for local government control.

Free municipal campgrounds built and maintained by towns along major roads proved a satisfactory alternative until town-and-town competition sharpened and budgets ballooned. Rising municipal costs soon led to the imposition of fees on the campers, a practice that promptly engaged the attention of private capitalists to the business of accommodating highway travelers.

Business competition, in turn, soon centered on the improvement of sites, service, and facilities for those able to pay. "Thus," Belasco writes, "the motel industry was born" (p. 4), and with it what one auto camper called "hoboing de luxe."

During the business boom of the mid-1920s, when sanguine capitalists, in Belasco's words, "tired of carrying equipment," cabin operators added good beds, good linen, stoves, and indoor plumbing to their facilities. The "great crash" of 1929 and the ensuing long depression, rather than harming the business of these enterprises, only improved it by drawing to them many of the erstwhile well-to-do now willing to forego their normal grand hotel life. By the mid-1930s, trailers had become very popular on the road, reviving the practice of "gypsying" while promoting at the same time the rapid development of trailer camps.

Space does not permit any elaboration here of Belasco's grasp of all these innovations and tendencies and their deep social impact, but it is continuously evident nevertheless. As scholarly books go, his is a short one yet comprehensive enough to do full justice to the many facets of his subject. His readers will look forward to more from the same pen.

WILLIAM MILLER  
West Redding, Connecticut

FREDRICK J. DOBNEY. *River Engineers on the Middle Mississippi: A History of the St. Louis District, U.S. Army*

*Corps of Engineers.* Washington: Government Printing Office. 1979. Pp. vii, 177. \$7.50.

As a long-time participant in the U.S. Army's military history program, I am well aware that organizational histories of that service are sometimes less than satisfying. It was a pleasure, therefore, to read this rather slim monograph, which covers the history of the St. Louis District of the U. S. Army Corps of Engineers, and find it both visually pleasant and intellectually stimulating.

Frederick J. Dobney, a history professor at St. Louis University who accomplished the project under contract, has chosen the chronological approach to tell his story. And he tells it well. He begins by briefly recounting the formation, exploration, and settlement of the Mississippi Valley but quickly turns to the importance of the river in the growth of our nation, with emphasis on the influence of the St. Louis environs.

In pursuing his chronological approach, Dobney weaves the strands of natural phenomena (snags, regularization of the channel, flood control) with federal response and interest in the area. The latter was largely determined by political and economic factors. Throughout the narrative, we catch fleeting glimpses of the organizational machinery that governed engineer efforts. Ironically, given the subject of the volume, the organizational coverage is the weakest. We gain little knowledge of the reasons for organizational change or the relationship of the St. Louis District to Washington or to its sister districts.

The Great Depression and World War II brought new initiatives for the district, and the environmental concerns of the past two decades have added new responsibilities and problems.

The author has relied primarily on a wealth of secondary material, including the annual reports of the chiefs of engineers. Copious illustrations, both art and photographs, enhance the text. Although it is good to see what all the district engineers looked like, four of them on a page are sometimes too many. And it is disconcerting to see these officers anachronistically depicted as generals or West Point cadets without explanatory captions. The author includes all sorts of interesting data—commercial tonnage on the Mississippi and Missouri since 1824, steamboat accidents and fatalities, steamboat arrivals at the port of St. Louis. The index is brief and weak.

Is Dobney, a hired hand, objective in presenting the story of the St. Louis Engineer District? I think so. The most sensitive area is the most recent one, and his coverage is even handed. The author acknowledges the organizational conservatism of the Corps of Engineers, but this is tempered by a tradi-

tional tendency to adapt to the prevailing values of society. After all, the new Engineer slogan, The Corps Cares, did not emerge from a vacuum.

BROOKS KLEBER

*U.S. Army Training and Doctrine Command*

JOSEPH G. KNAPP. *Edwin G. Nourse—Economist for the People.* Danville, Ill.: Interstate Printers and Publishers. 1979. Pp. 544. \$11.95.

Appointed by Harry Truman in 1946 as the first chairman of the Council of Economic Advisors, Edwin Nourse confronted the pitfalls facing the economist practicing his profession in a political milieu. Nourse served on the Council only three years before he resigned his post because he was unwilling to adjust to the politics involved in formulating economic policy. Since then, the Council has experienced varying degrees of success in influencing economic policy.

Nourse will be remembered primarily as the first chairman of the Council and secondly as an agricultural economist and director of economic studies at the Brookings Institution before he entered government.

Nearly half of the book is devoted to Nourse's early life and career before coming to the Council. Joseph G. Knapp discusses Nourse's involvement in the cooperative farming movement and in the development of agricultural economics as a profession in the 1920s. Nourse's greatest work before entering government came during the New Deal when he undertook a major evaluation of the Agricultural Adjustment Administration and directed the Brookings Institution's influential four-volume study of the "capacity" of the American economy. Walter Lippman called this latter work the most useful economic study made in America during the depression.

The second half of the book is devoted to Nourse's work in the Council of Economic Advisors. Much of the material can be found in Nourse's *Economics in the Public Service* and in secondary accounts of the Council by Edward Flash and Hugh Norton. The author shows that Nourse carried into his work on the Council his firm belief in "non-partisan" economics and in the necessity of a balanced budget. Nourse made a subtle distinction between "advising" the President and "advocating" or defending current policies. He therefore refused to testify before the Joint Economic Committee; yet he advised Truman that the Marshall Plan was sound economic policy. Not enough discussion is offered, however, for the reader to judge the extent to which Nourse's attitudes were in fact political. Clearly Nourse advocated fiscal restraint as the best means

of controlling inflation, a policy considered outdated by liberals. In 1949, feeling his days on the Council were numbered following Truman's reelection, Nourse openly attacked the administration for fiscal irresponsibility. Truman accepted Nourse's resignation shortly afterwards. Although Nourse, like many other moderates, came to accept the huge budget outlays for defense that coincided with the Korean War, he continued to be haunted until his death by his fears of inflation.

Knapp undertook this project in 1966, eight years before Nourse's death in 1974, as a "monument to the memory" of a close personal friend. Knapp, who completed this work after retiring as an economist with the Department of Agriculture, based his research on extensive interviews with Nourse and his associates, as well as Nourse's published writing and some unpublished speeches. Little use was made of other available primary sources, however, especially those in the various presidential libraries, which have remained unexploited in other recent histories of economic policy as well. Knapp's study is rich in detail, although he frequently loses the interpretation in telling the story. The meaning of "economist for the people" is never explained. Another weakness is his failure to place Nourse in a larger context by describing the general economic conditions of the time or the views of other contemporary economists. Still, this first biography of Nourse tells the reader much and should fulfill the author's hope that his work will serve as a guide for further explorations into this relatively new historical field of public policy.

DONALD T. CRITCHLOW  
North Central College

DARLENE CLARK HINE. *Black Victory: The Rise and Fall of the White Primary in Texas*. (KTO Studies in American History.) Millwood, N.Y.: KTO Press. 1979. Pp. xv, 266. \$19.95.

The intensive study of black history, so astonishingly fruitful for the eras of slavery and Reconstruction, is producing a growing list of works on the twentieth century. *Black Victory* is a modest, useful contribution to that list despite a major problem of focus. "The Supreme Court's path to this position [i.e., invalidating the white primary]," Darlene Clark Hine writes, "and the political and social significance of the 1944 *Smith v. Allwright* case [in which that was accomplished] are the central foci of this study" (p. xi). In spite of this statement and of the subtitle as well, the core of the book is an account of the effort, chiefly of NAACP leaders and lawyers, to mount a successful legal challenge to the white primary laws of Texas. The real focus is on the maneuverings within the NAACP and between

that organization and local blacks, especially in El Paso and Houston. Efforts to find satisfactory cases and defendants, to secure competent local counsel, to raise funds, and to see the cases through lengthy appeals are treated in some depth. The most original and important sections discuss differences over tactics and strategy between local blacks and the NAACP, including the desire of the Texans for more involvement in the cases and for greater use of black attorneys. Hine is especially effective in describing the tensions these differences produced, and in this her study supplements earlier work by her mentors August Meier and Elliot Rudwick. This facet of the subject has not previously been detailed, and Hine's doing so is the chief contribution of her study.

The book is marred, however, by its imperfect focus. Major facets of the subject central to Hine's purpose are poorly treated or treated not at all. Constitutional issues are insufficiently delineated, and the precise implications of the various cases are not always intelligible. The results are a distorted reading of some of the decisions and a tendency to overstate their significance. It is difficult to agree, for example, that "the Supreme Court had served as defender of black rights" (p. 174) in *Nixon v. Herndon* and *Nixon v. Condon*, since those decisions rested on such narrow grounds that they produced no meaningful relief for blacks. It is equally difficult to agree that *Smith v. Allwright* "had a profound impact on jurisprudence and American politics" and was "the watershed in the struggle for black people" (p. 233; emphasis added). Certainly Hine does not substantiate these judgments.

Nor does she tell us much about other key aspects of her subject. The white primary, Hine writes, was "the most effective of the disfranchisement subterfuges" (p. ix), but she offers no voting or registration data before and after its advent or comparative data about the impact of other subterfuges to test that judgment. Nor does she detail the actual operation of the white primary in Texas (some blacks in some counties, she notes only in passing, were always permitted to vote as Democrats); nor does she explain how and why black Texans, most of whom were Republicans until the mid-1930s, came to focus their political and legal attention on the Democratic primary rather than on other forms of disfranchisement. There is still much to be written about the rise and fall of the white primary in Texas.

I. A. NEWBY  
University of Hawaii

PATRICIA DAWSON WARD. *The Threat of Peace: James F. Byrnes and the Council of Foreign Ministers, 1945-1946*.



Kent, Ohio: Kent State University Press. 1979. Pp. x, 227. \$12.50.

Patricia Dawson Ward's *The Threat of Peace*, the latest volume in Kent State University's studies in American diplomatic history, is the first serious treatment of the diplomacy of Secretary of State James F. Byrnes in the Council of Foreign Ministers since the publication of George Curry's *James F. Byrnes* in 1965 in the prestigious *American Secretaries of State and Their Diplomacy* by Robert H. Ferrell and Samuel F. Bemis. Up until then, historians of United States foreign relations in the immediate postwar era had perforce to rely on Byrnes's own account of events in *Speaking Frankly* and *All In One Lifetime* published in 1947 and 1958, respectively. At the Potsdam Conference held in early July 1945, the United States, the USSR, and Great Britain called into existence a Council of Foreign Ministers (plus France and China where appropriate) to draw up draft treaties for Italy, Bulgaria, Rumania, Hungary, Finland, and, hopefully, Germany. Byrnes served as the principal United States representative on the council from the abortive London meeting in September 1945 until his resignation from the state department in January 1947. Within this time frame and within the space of 179 pages and 8 tightly-written chapters, including a concluding chapter—the rest of the book turned over to extensive notes, a skimpy bibliography, and an adequate index—Ward “details Byrnes’s attempt to harmonize the conflicting Soviet and American positions at the six conferences of the Council of Foreign Ministers held during 1945 and 1946” (p. x).

Faced with tasks beyond his talents, as ignorant of foreign affairs as President Truman, and generally unprepared, “Byrnes,” according to Ward, “went to the London Conference of the Council of Foreign Ministers with only a vague conception of what the United States wanted written into the treaties, but with the firm assumption that American military and economic power would allow the United States to dictate the treaties” (p. 174). There, Byrnes encountered the stiff resistance of his Soviet counterpart, V. M. Molotov, who made it clear that neither atom bombs nor dollars would or could deter the Soviet Union from attaining its status as a victorious great power replete with treaties sanctifying control of the Balkans, large reparations, a share in the control of Japan, and bases in the Mediterranean—to name but several goals. By the time of the Moscow Conference in late 1945, Byrnes had shed his “benevolently arrogant expectation” (p. ix) of what Washington could force Moscow to do and instead shifted to a new foreign policy tactic of quid pro quo negotiations. But while relatively successful with the Soviets privately—for example, token concessions in Japan for

token concessions in the Balkans—publicly Byrnes blamed the Kremlin for the breakdown in the Grand Alliance, thus feeding the growing demand in 1946 “to get tough” with the Russians. On the other hand, Ward finds Byrnes himself guilty of diplomatic incompetence, for while it is true the secretary was instrumental in helping to realize the peace treaties (less Germany of course), it is also true that he was responsible for encouraging “the impetus toward the cold war” (p. 179). And so yet another postwar American secretary of state must bear the burden of history for failing to recognize openly the “realism” and “legitimacy” of allowing the Soviet Union to dominate the bulk of the Eurasian landmass.

Methodologically speaking, *The Threat of Peace* exhibits more holes than a slice of Swiss cheese. For one thing, in her obsession with apportioning guilt, Ward, at her own peril, has replaced the traditional historicizing question, “How it actually was,” with the very different question, “How was it possible for it to come about?” Such an approach could only vitiate the historical worth of her effort. For another thing, Ward fails to adduce a scintilla of evidence that Soviet goals “were more realistic than those of the Americans, and [that] the treaties reflected reality” (p. 176). Of whose reality does she speak? Moreover, Ward cannot begin to prove that “Molotov manipulated the others [in the council] like a puppet master” (p. 176), which is not to say, however, that the Soviet foreign minister did not know how to play the puppet himself. Of course, the trail here leads straight back to Stalin, and he thought Byrnes, if the story in the *Henry Wallace Diary* can be taken as a guide, “the most honest horse thief he had ever met.”

In any case, the Council of Foreign Ministers still awaits its historian. Perhaps the best that may be said of *The Threat of Peace* is that it will one day encourage others to come to grips with this truly unique diplomatic experience in a more objective manner.

JOSEPH M. SIRACUSA  
University of Queensland,  
Australia

JUSTUS D. DOENECKE. *Not to the Swift: The Old Isolationists in the Cold War Era*. Lewisburg, Pa.: Bucknell University Press. 1979. Pp. 289. Cloth \$17.50, paper \$8.95.

Justus D. Doenecke's book bristles with insights about the persistence and permutations of American isolationism in the first decade of the Cold War. He surveys reactions by foes of American embroilment in World War II to a series of Cold War developments, among them Bretton Woods, the UN,



the 1946 British loan, atomic diplomacy, aid to Greece and Turkey, the Marshall Plan, NATO and the military assistance program, various German issues, the fall of China, the Korean War, and the 1954 Indochina crisis. The isolationists' role in the 1948 and 1952 elections, in the McCarthy controversy, and in the fight over the Bricker amendment is also detailed.

The isolationists, Doenecke believes, have much to teach us. Their objections to globalism, the growth of executive and military power, and policies provocative to the Soviets were often prescient. Doenecke defines isolationism chiefly in rational ideological terms. He is less impressed with sociological or geographical interpretations, but his approach is eclectic. He sees a strong anti-urban bias among isolationists and, in their efforts to restore the pristine virtue of a lost polity, a resemblance to Bernard Bailyn's revolutionaries. Doenecke examines both politicians and publicists; his focus is broad, intellectual as well as political.

Notwithstanding his respect for the old isolationists, Doenecke is remarkably critical of them. In many isolationists, dread of communism clashed with fears of interventionism to produce an unstable mix. Doenecke captures their erratic and inconsistent moments and the bizarre and phobic elements of their beliefs. Thus, William Lemke warned that with the Truman Doctrine Uncle Sam had deserted "beautiful Miss Columbia" to philander after "red, pink, green and off-colored skirts all over the world" (p. 81).

Doenecke has rescued the isolationists from the limbo to which the victorious interventionists consigned them. Partly he achieves this by demonstrating their perceptiveness, but, more interestingly, he shows how elements of their supposed heresy found their way into the mainstream of policy. As the Truman administration's German and Asian policies evolved, they came to resemble isolationist prescriptions, as did the nation's increasing resort to a defense based on air power and nuclear deterrence. In this sense, the defeat of isolationism was equivocal, and the label itself was inexact and transitory, as indicated by the fact that John F. Kennedy and Gerald R. Ford were once contributors to the America First Committee.

The author's analysis is cogent, balanced, and invariably suggestive. One might quibble that a more rigorous quantitative analysis of the isolationists in Congress would have been rewarding. Additional manuscript collections, including the papers of Norman Thomas, William Langer, and Karl Mundt, might have been profitably consulted. Then too, one of Doenecke's major achievements, the elaboration of the full spectrum of isolationist views, poses a problem: Senator Robert Taft and other potentates are juxtaposed with publishers of interesting

but little-read newsletters; the precise center of gravity of this motley group is hard to locate. However, these are minor criticisms of a well-researched, subtly argued, first-class piece of scholarship.

RICHARD M. FRIED  
University of Illinois,  
Chicago Circle

JANE SANDERS. *Cold War on the Campus: Academic Freedom at the University of Washington, 1946-64*. Seattle: University of Washington Press. 1979. Pp. xii, 243. \$12.50.

In 1949, the Board of Regents of the University of Washington, rejecting the recommendations of a divided faculty hearing panel, dismissed three tenured professors for their avowed or presumptive membership in the Communist Party of the United States. An outgrowth of the revelations of the Canwell Committee (the first of a rash of state legislative inquiries into subversion that adopted the techniques of the House Un-American Activities Committee), this action by the Regents was followed by a series of aftershocks—repeated banning of controversial speakers from the campus rostrums, labeling of professors who belonged to so-called "front" organizations, appending of a disclaimer affidavit to the pledge of loyalty required of all employees—that roiled the university for decades and earned it the reputation as the place where academic freedom buckled under Cold War stress. Jane Sanders, who was a student and teacher at the university during the closing phase of its long ordeal, tries to fix the blame for the woes that befell her alma mater, relying on institutional records that had not heretofore been used for this purpose and on the detached perspective that is presumed to emerge from a lapse of time.

Only in a limited sense can it be said that she is successful. She does show, by delving into local history, that much of the hue and cry against subversives was prompted less by concern for the nation's security than by personal feuds and party conflicts—for example, the efforts of Dave Beck, vice-president of the Teamster's Union and a regent, to settle old scores with a radical labor enemy, Harry Bridges; the desire of certain Republican politicians to gain votes by stigmatizing their opponents; and the fierce ideological conflicts that had long been waged within the Democratic camp. She also shows, by quoting several presidents against themselves, that nothing stood higher in the administrative scale of values than securing the next year's appropriations and that they would veto acts of surpassing harmlessness, like asking J. Robert Oppenheimer to give a lecture, rather than give any jaundiced keeper of the purse offense. But aside

from adding colorful detail to the propositions that superpatriotism is the last refuge of petty scoundrels and that presidents of state universities are a cautious lot, the author contributes little to our understanding of these events. She offers no cultural or structural comparisons that would help us see why Michigan and Wisconsin, not to mention Chicago and Harvard, fared better than Washington or California in those dark times. She brings scant technical knowledge or paraphrastic skill to her discussion of the constitutional and legal issues: on this score, Vern Countryman's contemporary account of the Washington debacle remains a more useful, if truncated, work. And—a more serious fault—she greatly oversimplifies the moral and professional issues that were raised when American Communists in the forties and fifties pressed their academic freedom claims.

In the author's view, the chief responsibility for what went wrong rests with liberal academics on and off the campus—with faculty organs of opinion that split over the crucial question of whether Communists were fit to teach; with the builders of academic departments at UW who longed for national recognition and were willing to trade silence for supporting funds; and with the national AAUP, which was so torn over the Communist issue that it took six years to report on the Washington dismissals and thus forfeited the opportunity to carry weight. This failure-of-liberal-nerve hypothesis does not pass key empirical tests. The national AAUP did not depart from its early pronouncement that membership in the Communist Party should not in itself be grounds for dismissal; it failed to issue a prompt report for the same reason that it failed to answer its mail—its general secretary had become incapacitated but was able to fend off internal criticism until at last he was replaced. On the author's own evidence, those who acceded to oaths and inquisitions were not "cosmopolitans" but "locals"—the faculty of the schools of business, forestry, and engineering. Here, as doubtless elsewhere, the courage to stand up to local pressures was fanned by the ambition to stand high among distant peers. Finally, she misconstrues the character of the Communist Party in this period and thus passes judgments on its critics that are uninformed and overharsh. To treat the view that the party was a Russian tool as an example of the dementia of that moment, to write a book about the Cold War on the campus without once mentioning Stalin or Stalinism, is to revise, not recall, the past. To fail to see that civil libertarians could also be anti-Communist—that they could regard the party as totalitarian but not every member as totally committed to it or oppose incontinent exposure at home while resisting Soviet moves abroad—is to rule in favor of simple minds. And to hold the faculty culpable because it was not "unan-

imous" is both to minimize its persistent and ultimately successful fight for academic freedom and to demand a measure of conformity that negates what academic freedom is about.

WALTER P. METZGER  
Columbia University

DONALD R. MCCOY. *The National Archives: America's Ministry of Documents, 1934–1968*. Chapel Hill: University of North Carolina Press. 1978. Pp. ix, 437. \$19.00.

In less modest moments, of which there are depressingly few, archivists claim to be as important to scholarly research as are bankers to the nation's economy: both hold the keys to the vault of resources. Donald R. McCoy's history of the National Archives and Records Service is particularly valuable to those scholars who rely on NARS for the resources that make their research possible.

Although McCoy apparently had the full cooperation of NARS, his product is not court history in the pejorative sense. He used NARS records, interviewed NARS personnel, and submitted his manuscript to NARS officials; but he does not hesitate to point out NARS flaws. Neither is the book institutional history in the derogatory sense. McCoy does not dull the senses with a dry narrative reconstruction of organization charts and statistics. Rather, he injects life into the volume by organizing his tale around the first four Archivists of the United States.

McCoy begins his account with the 1934 National Archives Act, which established the National Archives as an independent federal archival agency. The archives became part of the General Services Administration in 1949, where it was rechristened the National Archives and Records Service. McCoy concludes with the 1968 appointment of James B. Rhoads as Archivist of the United States, symbolizing for McCoy a new generation of institutional leadership. Coincidentally, the volume's publication preceded Rhoads's unexpected retirement by only a few months.

Three themes emerge of special importance to historians. The first concerns NARS's dual role. Is NARS an educational and cultural institution serving the public's right to information about its democratic government? Or does NARS serve the federal government's desire to minimize the huge cost of federal records creation, storage, reference, and disposition? Although the National Archives was established primarily for the former reason, its executives quickly found that the latter justification generated more funds from Congress. McCoy argues that NARS should continue to fulfill both functions, albeit independently of the General Services Administration.

A second theme of direct consequence to historians is restriction of access to federal records. NARS originally acted as a neutral observer of this process, simply passing on the bad news from agency to researcher. McCoy credits this tack with allowing NARS to persuade federal agencies to turn over their records, not an easy task in the early years of NARS existence. He also acknowledges that researchers were slow to bring pressure on NARS to negotiate better bargains with the agencies. As researchers have become more assertive in recent years, NARS has become a more forceful advocate of researcher access.

The third theme is the leading, though intentionally muted, role NARS has played in the development of the archival profession through the Society of American Archivists and its journal, *The American Archivist*, and the International Council on Archives, with a journal currently in gestation. This nonstatutory contribution to scholarship has improved research opportunities in archives throughout the United States and around the world.

MARTIN I. ELZY

*Lyndon Baines Johnson Library*

MARK H. ROSE. *Interstate: Express Highway Politics, 1941-1956*. Lawrence: Regents Press of Kansas. 1979. Pp. xii, 169. \$14.00.

That the "highway lobby" was influential in promoting American road building comes as no surprise to anyone. That this lobby contained a variety of special interests that differed on many basic points and that the resulting confusion and disagreement effectively deterred legislation for a national system of highways is, however, something new—and the subject of this book.

The greatest pressure for roads came from an explosion in the numbers of motor vehicles that no government could long ignore. Particular groups also touted highways, but they hardly agreed on purposes and methods and often reflected local biases and narrow prejudices. Farm organizations concentrated on rural market mileage, planners were interested in shaping and reshaping cities and accomplishing a variety of social objectives, truckers wanted to increase profits and minimize costs, and engineers were concerned almost solely with traffic movement and efficiency. Economists looked first to whatever promised the greatest economic growth.

Commissioner of Public Roads Thomas H. MacDonald's proposal for the 40,000 mile national interstate expressway system was included in the Federal Highway Aid Act of 1944 but was not allocated a portion of the roads budget. As elements of the highway lobby feuded over construction priorities and methods of financing—tolls, bonds, increased user taxes—the project seemed doomed to endless

controversy. Lucius D. Clay, tapped by President Eisenhower to coordinate a new federal road-building program, struggled to forge a consensus with no greater success.

The Interstate Highway Act of 1956, introduced by Congressmen George Fallon and Hale Boggs, finally succeeded because it satisfied the long-term goals of every major highway interest group (except, perhaps, farmers), required few sacrifices, did not drain the Federal budget, and avoided difficult issues. This legislation was not, Mark H. Rose emphasizes, the product of constructive give-and-take among the highway interests themselves. "References to compromise, at least in highway politics," he writes, "served as functional myth" (p. 89). The act provided for 90 percent Federal financing, a Highway Trust Fund, and no substantial new taxes on truckers or other highway users. Thus was the biggest public works project in human history begun.

This extensively researched, brief, and important study adds to our knowledge of interest group politics and the impact of the motor vehicle in the United States. While hardly the final word on all aspects of the subject, it provides a useful framework for further investigation and contains especially interesting information on urban highway planning. It also reminds us that assessments of the impact of technology that omit economic interests and political decisions from the list of variables are exercises in futility. Ironically, at no time, by any group, was the cost and availability of petroleum mentioned as a significant issue.

BLAINE A. BROWNELL

*University of Alabama,  
Birmingham*

## CANADA

DOUGLAS MCCALLA. *The Upper Canada Trade, 1834-1872: A Study of the Buchanans' Business*. Toronto: University of Toronto Press. 1979. Pp. viii, 231. \$15.00.

This lucid and valuable case study, based on company records and a wide and varied range of primary sources, traces the rise and fall of an important wholesaling firm based in Glasgow, Scotland, and in Hamilton, Upper Canada, during the prime period of the commercial and economic growth of what was to become the province of Ontario. Douglas McCalla's name is already well known in Canadian business and economic history circles through his constructive contributions in the field and through his able commentaries on such recent works as that of Tom Naylor. This book provides a detailed and able analysis of a key firm in its line—

precisely the type of groundwork study that is needed before attempts are made to write a general history of Canadian business enterprise.

The Buchanan brothers, Peter and Isaac, launched into the Canada trade in 1830, after disappointments in the failing West India trade. Their line, operating first in Montreal, was essentially the wholesaling of dry goods imported from Britain, but Isaac, the younger brother and manager at the Canadian end of operations, resolved earlier that Upper Canada was the more promising venue of operations and moved to York (Toronto) in 1832 and, in 1834, to the then frontier area of Niagara and Hamilton, which was to become his hub. A man of "vision and enthusiasm," he was energetic and full of ideas for the development of the area. As an "ideas man" he was soon heading the local business community, building up an extensive clientele among storekeepers and settlers over a wide area, and making handsome profits averaging 30 percent on the initial capital of £25,000 that included, in characteristic Scottish fashion, a "cash credit" of some £5,000 from the Glasgow Banking Company. Traveling constantly, winter and summer (he disliked office routine), he built up his business, attributing the success of the firm to picking Scotsmen as his customers, distrusting "Yankees," and accepting the "Canadian born" only "after the most vigorous scrutiny." The Buchanans, in fact, acted as business consultants to their storekeeper clients, and in these early years of the 1830s and 1840s their profits increased steadily. Unfortunately, the ebullient Isaac became involved in politics and in the Protection issue, in railroads, and in other diverse and time-consuming activities. Chapter 3 ("Success in Toronto") presents a fascinating picture of the firm's working methods and of the manner in which Upper Canadian trade at city, town, and country levels was carried on. Isaac's belief in his own judgment led to further ventures into ironware and pig iron importation and into tea, tobacco, and other importations through New York. The fact that the firm had made a sixfold increase in its capital by 1845 may have seemed to justify this—the company was the biggest wholesaler in Canada West by this time—but speculation in Upper Canadian wheat and involvement in the promotion of the Great Western Railroad in the early 1850s sapped away the partners' time, energies, and attention to their basic business of wholesaling dry goods. By the crisis of 1857 the firm lacked liquidity of funds, and far too many of their storekeeper customers were irrecoverably in debt to them.

McCalla skillfully links these booms and recessions of the time to the firm's changing fortunes. The portrayal of Isaac's character, problems, and essential weaknesses is balanced and judicious, albeit sympathetic, for this was an *impulsive* though creative businessman. The sad and tangled tale of

the final bankruptcies and of the attempts at reorganization is well told. The promising town of Hamilton lost ground as railroad trade confirmed the position of Toronto and Montreal as Canada's main importing and distributing centers. As the Upper Canadian wheat-growing boom faded in the 1860s, more specialized trading firms replaced the older style of Buchanans' wholesaling. Isaac's "perennial optimism" was no counter to the more effective and tightly controlled credit management of his rivals.

Throughout the analysis there is a clear exposition of how and why decisions were made, while the competitive conditions, the business and economic climates against which they were made, are clearly shown. There is a whole series of useful, well-designed tables recording sales, shipments, and profits for most of the period. Altogether this is a precise, perceptive study of a great firm's rise and fall.

DAVID S. MACMILLAN  
Trent University

BRYAN D. PALMER. *A Culture in Conflict: Skilled Workers and Industrial Capitalism in Hamilton, Ontario, 1860–1914*. Montreal: McGill-Queen's University Press. 1979. Pp. xviii, 331. \$10.95.

A book like this shows why the new labor history has become an emotional subject. Bryan D. Palmer tries to describe and explain the culture of the skilled workers of Hamilton, Ontario, in the decades prior to World War I. He intends to show how and why they struggled for control over their lives, homes, and workplaces and how they evolved ideologically. The book gives us a fuller picture of these workers than we have yet had, along with a useful summary of the course of industrial conflict in southern Ontario during these years. Palmer's analysis (done with Peter Warrian) of the evolution of reform thought in the Hamilton area is also valuable. This book can teach us much about skilled workers in Hamilton.

But Palmer does not leave it at that. As a practitioner of the new labor history, he believes the only way to analyze the actions and attitudes of workers is to study their culture. Here the book is weakest. Palmer's discussion of working-class culture, for example, tries to show how the "associational" life of Hamilton workers contributed to a sense of solidarity. He describes friendly societies, drinking clubs, and sports teams that had workers as members, but he cannot describe the extent of their participation. He admits that membership data are "extremely rare" (p. 41). Aside from two societies he "can only speculate" (p. 42) about the extent of worker participation. This is even true of the Mechanic's Institute. When discussing charivaris and whitecapping, Palmer admits that participants in such events "do



not lend themselves easily to identification." Even so, he goes on to exclaim that "it would be strange indeed if a crowd of 300 young men, girls and boys gathered on Hamilton's street corners on a Sunday afternoon contained no workers" (p. 65). Palmer thus presents no real evidence that most of the culture he describes is any less that of the banker or the shopowner.

So, too, with his treatment of control. The "culture of control" (p. 71) was a culture of "autonomous workers," he asserts (p. 222). These were "manly" artisans (p. 85) smoking and drinking their leisure time away (p. 91), controlling their pre-industrial work environment. But there is no proof that the authority of the owners stopped at the shop door. Even then employers had the basic power to hire and fire and to set the size of their workforce. Palmer describes many battles fought over control issues, but closer examination shows they were almost always not control but economic struggles. Palmer admits this (p. 91) but claims that these struggles formed a context for more traditional control battles. We are left to wonder if Palmer has arbitrarily dispensed with economism in favor of control as a general category into which all strikes and lockouts should somehow fit.

In two places Palmer generalizes too much. The discussion of reform thought in Hamilton is excellent. But Palmer (and Warrian) then claim, without proof, that this was the reform thought of the whole "Canadian working class" (p. 122). Palmer later asserts that class unity had developed in Hamilton by 1914 (p. 227) but then shows in the following pages that this unity was only that of the English skilled workers.

This book could have used a strong editorial hand. Phrases like "matrix of control mechanisms" (p. 128); "contours of the conflict" (p. 130); "synthesis that transcended the fragments constituting the whole" (p. 154); and "web of legitimization" (p. 212) abound.

The new labor historians are dedicated to rescuing the working people from obscurity. This book does move in that direction. But Palmer's evidence to support key parts of his analysis is thin, and his book contains too many generalizations. Surely the rescue work should begin with old-fashioned empiricism.

DAVID J. BERCUSON  
University of Calgary

#### LATIN AMERICA

SHERBURNE F. COOK and WOODROW BORAH. *Essays in Population History*. Volume 3, *Mexico and California*. Berkeley and Los Angeles: University of California Press. 1979. Pp. xiii, 333. \$20.00.

This last volume in their series, *Essays in Population History*, closes a quarter century of fruitful collaboration between Woodrow Borah and the late Sherburne F. Cook, a rich collaboration that has made groundbreaking contributions to the demographic and socioeconomic history of Spanish America, provided methodological models and techniques for historical demographers, and opened up heated historical debate. This book should do the same.

In the first of their three essays the authors attempt to establish the low point for the Indian population of New Spain in the seventeenth century. Relying on new documentation on royal revenues and tribute assessments in the Audiencia of Mexico for 1646 uncovered by E. William Jowdy, Cook and Borah compare this new count with those already extant for 1568 and 1595. Establishing ratios for the Indian population in *pueblos* found in ten regions of Mexico documented in all three counts, they calculate that the indigenous population fell to its lowest ebb ca. 1620–1625, the adjusted average date for tribute assessments found in the 1646 document and a "reasonable compromise" for the low point of the Indian population. They conclude that ca. 1620–1625 approximately 730,000 Indians lived in the Audiencia of Mexico or 3 percent of the population in 1519.

The second essay is a remarkably informative piece on pre- and post-conquest Indian diet and food production, an analysis demanding digestion and an integration of literature on nutrition, ecology, physiology, biophysics, and a number of other disciplines. Admittedly tentative—but convincing—their findings show that the pre-Columbian Indian had a meager but diverse diet of maize, *frijoles*, squash, chiles, *chia*, *hualtli*, agave, cactus apples, ducks, dogs, turkeys, algae, and perhaps human flesh. Per capita daily intake based on the average height, weight, and metabolic rate of an adult Indian male between fourteen and forty (159 centimeters, 53.6 kilograms, and 1,425 kilocalories) was somewhere between 1,400 and 1,800 kilocalories daily, sufficient for sustaining a good deal of physical effort but considerably less than the consumption of most European and American laborers today. Based on production of 700 to 1,200 kilograms of maize per hectare, only 10 to 15 percent of the land available needed to be under production to feed the population in 1519.

The last essay demonstrates how much can be gleaned from the registers of eight California missions and sets up models of what might be done also with the registers for the other fifteen. Among other things Cook and Borah provide information on ages at baptism, life tables for specific cohorts, ratios of birth to adult females, population statistics on non-Indians, and data on racial mixing for the late eighteenth and early nineteenth centuries. They conclude also that the Hispanic residents of the Alta



California missions were "as robust, as long-lived, and as fecund as the *habitants* of French Canada."

As in all their collaborative work, these two thoughtful, innovative scholars meticulously lay out their assumptions and methodology; explain their sources in detail; crosscheck with other data wherever possible; and candidly assess the degree of reliability of their conclusions. Based primarily on documents, the first and third essays will probably elicit little debate, but the second, once again justifying high estimates of the Indian population in 1519, will open up new controversy, but controversy that should bring us closer to historical reality, the overwhelming goal of all of Cook and Borah's research.

JOHN J. TEPASKE  
Duke University

THOMAS L. KARNES. *Tropical Enterprise: The Standard Fruit and Steamship Company in Latin America*. Baton Rouge: Louisiana State University Press. 1978. Pp. xv, 332. \$20.00.

Central Americanists have long craved a solid history of the activities of the transnational banana companies in Central America and the Caribbean. They have particularly hoped for a study of the most important and most controversial of these firms, United Fruit. United Fruit remains a somewhat mysterious and controversial factor in Central America, but in the meantime Thomas L. Karnes's *Tropical Enterprise*, the best historical study of the banana industry yet to appear, provides considerable food for thought.

The focus of the book is on the Standard Fruit and Steamship Corporation and its principal predecessor, the Vaccaro Brothers and Company. It narrates the resolute efforts of the Vaccaro and d'Antoni families, starting in 1899, to establish themselves in Honduras for the purpose of selling bananas in New Orleans. It records the details of their success, despite the competition of the larger and more diversified United Fruit, in making Honduras the leading banana-exporting nation of the world by the mid-1920s, when the families incorporated as the Standard Fruit and Steamship Company. Although Honduras and New Orleans hold center stage, there are highly informative chapters accounting for the company's forays into Nicaragua, Mexico, and Haiti. The narrative closes with an account of Standard's overtaking of United in the post-World War II period and the negotiations and purchase of the company by Castle and Cooke in the mid-1960s.

Perhaps the most important contribution of Karnes's study is the light shed on the controversy about the banana companies' unwarranted interference in host country politics. However true it

may be in other cases, this accusation does not appear to apply to Standard Fruit. The Vaccaro brothers established a clearcut policy of neutrality toward Honduran politics and identification with the region of their operations; the company managed to make profits in Honduras no matter which party was in power and despite unstable political conditions. Furthermore, Standard expressly accepted the Calvo clause, prohibiting business requests for diplomatic intervention, and seemed little inclined to ask for aid from the United States government. The Standard formula, so successful in Honduras, did not work in Nicaragua, Mexico, or Haiti, but the reasons for the failure lie more in local conditions than in company policies. Karnes does not expressly weigh the benefits and costs of the presence of the banana companies in any of these countries, but his conclusion is clear enough. Summing up Standard's decision to pull out of Haiti after a history of harassment, Karnes states: "The tragedy is Haiti's" (p. 259).

Despite the successful narrative and its substantial contribution, it needs to be said that Karnes appears to be insensitive to some of the larger issues involved in the international business scene. Surprisingly, he ignores the dependency argument and literature altogether. Furthermore, there is a notable neglect of the many Central American studies critical of North American companies in general and Standard Fruit in particular. Company papers and United States archival material are the principal sources used—leading to the heavy focus on the company rather than on its impact on the host countries. The fact that there is no bibliographical entry later than 1974 helps to account for this weakness.

CHARLES L. STANSIFER  
University of Kansas

MAGNUS MÖRNER. *Perfil de la sociedad rural del Cuzco a fines de la colonia*. Lima: Departamento Académico de Ciencias Sociales y Políticas, Universidad del Pacífico. 1978. Pp. xi, 186.

No other non-Spanish European historian has done more in recent years for the elucidation of social change in colonial Peru than Magnus Mörner. This thin but well-illustrated and carefully documented examination of a small area of the Viceroyalty of Peru during a period of approximately a century, from 1689–90 to 1786, is a testimonial to his continued interest in racial evolution and class consciousness in Spanish America. It is also indicative of a trend among Hispanicists toward microscopic scholarship in preference to the formulation of broad generalizations.

Why was the intendancy of Cuzco selected for this investigation? The answer is multifold. Al-

though endowed with a smaller population and administratively less significant than some other areas, the region was strategically located on the route between Upper and Lower Peru. It was the ancient center of the Inca civilization. More important, there were ample sources available in the Archives of the Indies and in Peru. The author's aim is to conduct a demographic and economic analysis of two exceptionally full and generally reliable founts of census data, the reports, or *informes*, compiled for the bishop of Cuzco of the parishes of his diocese in 1689–90 and a set of *informes* assembled in 1784 at the order of the intendant of the district. Hence the treatment is mainly topical. The coverage is comprehensive, with chapters on population, haciendas, production, communication, and exploitation and rebellion.

Unquestionably, the author's conclusions expand our knowledge of diverse social and economic conditions in the late colonial and pre-independence eras. Population did increase, though slowly, in an economy marked by the interaction of the capitalist tenets of the *hacendados* and the "archaic, passive, and isolated economy of the Indian pueblos, direct successors of the Incaic ayllus" (p. 156). Surprisingly, mestizo proliferation took place as rapidly in the remote districts as in the more populous areas.

With respect to the great Indian uprising of 1780, the author's views are revisionist. Discounting somewhat the iniquitous labor institution of the *mita* and the unfair distribution of goods by the *corregidores* as general causes, he cites the depressed status of the system of transportation as a specific factor. In his opinion, and perhaps rightly, the loss of life in the region was less than traditional estimates, and the impact on the general economy was more complex than hitherto supposed.

Despite minor shortcomings, this is a valuable contribution to the history of colonial Peru. The inclusion of comparisons with contemporary conditions in other areas of the Spanish Empire might have disclosed similarities or contrasts. The omission of a table of contents and the lack of a satisfactory index are regrettable. One might wish that the appearance of this study would encourage the undertaking of other works of a limited geographical and chronological nature as a foundation for future syntheses.

J. PRESTON MOORE  
Louisiana State University

SILVIO ZAVALA. *El servicio personal de los indios en el Perú (extractos del siglo XVII)*. Volume 2. Mexico: El Colegio de México. 1979. Pp. vii, 299.

Perhaps no problem is more central to an understanding of colonial Spanish American history as the persistent and agonized attempts of the crown

to successfully coerce Indian labor without simultaneously destroying the basis of native economy and society. Into this complex equation went the imperial perception of economic advantage and social justice, the aspirations of the conquerors and their descendants, the cold abstractions of demography, the dictates of Catholic morality (difficult for even the most subtle of casuists to circumvent), and the resistance, both passive and active, of the natives. Indeed, an entirely satisfactory solution to the problem was never arrived at, though it is safe to say that the conquerors profited more in the midst of their ideological vexation than did the conquered.

Silvio Zavala has devoted decades of valuable research to the problem, and his latest volume concerns seventeenth-century Peru, which witnessed an abortive effort to abolish the dreaded *mita* for the mines and other Spanish economic enterprises dependent on forced Indian labor. The scope and content of the work are made explicit by the title. This is an impressive compilation of extracts from archival sources and printed documentation and commentary, supplemented as appropriate by modern scholarship, on the changing status of what the Spanish chose to call the "personal service" rendered to them by the Peruvian Indians. Particular and welcome attention is paid to the supremely important silver-mining area of Potosí. No serious scholar of the period can afford to ignore a work that explores in such exquisite detail the efforts of a war-weary bureaucracy and theocracy to cling to the benefits of America's richest colony with consciences unscathed.

But a word of warning to the reader. Zavala has chosen to let his characters speak almost entirely for themselves; to the extent that Peru's achievements and ills during the seventeenth century are analyzed, we rely on no one but the participants.

FREDERICK P. BOWSER  
Stanford University

JESÚS CHAVARRÍA. *José Carlos Mariátegui and the Rise of Modern Peru, 1890–1930*. Albuquerque: University of New Mexico Press. 1979. Pp. xi, 247. \$14.95.

Peruvianists in this country and elsewhere have awaited the publication of this book for many years, and, happily, they will not be disappointed. Although there are a number of studies of the life of José Carlos Mariátegui and his influence on Peruvian and Latin American thought, none equals in depth and comprehensiveness this volume by Jesús Chavarría.

Chavarría begins with a cogent analysis of the economic and political changes that occurred in Peru, 1870–1919, and then moves to a treatment of the impact this period of modernization had on Peruvian intellectuals (for example, Manuel González

Prada, the lawyer-professors at San Marcos University, and the "Generation of 1900"). The author is at his best when dealing with changing intellectual currents, but he is equally adept at explaining economic and political events.

In the first two chapters, Chavarria succeeds in providing the reader with a clear understanding of and feeling for the Peru in which Mariátegui grew up and began his career in journalism. The next two chapters trace Mariátegui's early working years and his increasing politicization that culminated in 1919 in his being exiled to Europe. Here, Chavarria is particularly effective when comparing Mariátegui's "Generation of 1919" to that of "1900" and analyzing the impact of European (primarily Italian) leftist political thought in the late 1910s and early 1920s on Mariátegui's political development. The section on Mariátegui's return to Peru and his involvement in the newly established Universidades Populares de González Prada is also well done.

The final four chapters treat the last seven years of Mariátegui's brief but incredibly productive life, and it is here that Chavarria makes his most important contributions to Peruvian historiography. Chapter 5 deals both with the founding and impact of Mariátegui's influential journal *AMAUTA* and with the initial friendship and then bitter conflict between Mariátegui and Víctor Raúl Haya de la Torre and the fledgling APRA party. Chapter 6 provides an excellent analysis of Mariátegui's monumental *Siete Ensayos de Interpretación de la Realidad Peruana*, while chapter 7 constitutes the most valuable critique available of Mariátegui's highly eclectic, revisionist brand of Marxism. In the last chapter Chavarria describes Mariátegui's radical *indigenista* nationalism and persuasively argues that it was this that really set Mariátegui apart from his contemporaries in Peru and from the international leftist movement in general.

In addition to being solidly researched, the book is well written with the result that Mariátegui's ideas are elucidated with a clarity all too rare in intellectual history. My only substantial criticism is that neither the notes nor the bibliography include citations to the more recent scholarship on Peru, but that should not detract from the importance of this book.

THOMAS M. DAVIES, JR.  
San Diego State University

EUL-SOO PANG. *Bahia in the First Brazilian Republic: Coronelismo and Oligarchies, 1889-1934*. (Latin American Monographs, second series, number 23.) Gainesville: University Presses of Florida. 1979. Pp. xvi, 256. \$14.75.

Eul-Soo Pang has undertaken an analysis of a major area of Brazil in which he reveals how a system

of politics designed to accommodate various socioeconomic models demonstrated resiliency under the impact of national and international change. Pang's work deals with the full complexity of factions, varieties of "colonels," and regional diversity that characterized the historical processes depicted. An ambitious, if not monumental, research effort was required to provide the nuances involved in this study. Although the book is primarily concerned with the years 1889-1934, other areas and periods are touched upon to round out the picture. One result of this study is the laying to rest of certain stereotypes and myths such as the image of the "colonel" as always being a landowner or the notion that land tenure was a predominant factor in *coronelismo*. The figure of Ruy Barbosa receives a well-deserved rectification as a politician dedicated primarily to self-aggrandizement.

It is in dealing with developments in the *sertão* region that Pang excels. When introducing factors from the national scene, he sometimes falls victim to some inaccuracy. The overthrow of the Franco Rabelo regime in 1914 provides an example. This incident was related to the "military salvations" that Pang sees as "rescuing northern politics from the hands of the ruling oligarchies that Pinheiro Machado controlled." The "military salvations" scheme was national in scope and reached its climax when the minister of war announced his intention to serve as "military savior" in Rio Grande do Sul (Pinheiro's home state). President (Marshal) Hermes was forced to choose between his military comrades and Pinheiro Machado for support. He chose the latter, and the showdown in Ceará occurred when the Acioli-Cícero forces (aided by Pinheiro) toppled the regime of "military savior" Rabelo. The Ceará incident was just another phase of Pinheiro's victory over the military saviors. Pang sees it as some type of defeat for Pinheiro with his prestige "reaching a new low." Moreover, this cooperation between the national government and colonel-type forces possibly amounted to an antecedent to the phase of politics of the Old Republic that Pang characterizes as the "politics of presidents," which he believes began with the administration of Arthur Bernardes.

All in all, Pang has produced an admirable study that should be considered essential reading for all who would understand the evolution of that area of Brazil before, during, and after the period of the Old Republic.

ROBERT A. HAYES  
Texas Tech University

ENRIQUETA E. MOLINE DE BERARDONI. *Historia de Marcos Paz: Desde sus orígenes hasta la creación del Partido, 1636-1880*. La Plata: Archivo Histórico de la Provincia de Buenos Aires. 1978. Pp. 184.

Although her work lacks themes or interpretation, Enriqueta E. Moline de Berardoni has assembled through extensive archival research intriguing data on the growth of the town and *partido* of Marcos Paz (48 kilometers southwest of Buenos Aires city). She thus provides us with a microscopic view of shifting patterns of land ownership and of rural change in the decades that preceded the great national economic boom of the 1880s.

Through eleven brief chapters the author traces the evolution of Marcos Paz from 1636, when the first land grants in the area were made, to 1878, when the *partido* was created. In chapter 1 she describes the main geographical features of the zone encompassed by Marcos Paz and traces rapidly the major legal changes in its status through 1878. The remaining chapters chronicle the process of ever greater subdivisions of the land (chaps. 4–11), primarily between 1830 (the beginning of the Rosas dictatorship) and 1880 (the inauguration of Julio A. Roca's presidency).

In various sections the author gives us glimpses into political and economic matters, early town planning, and rural life. The heart of this work, however, lies in the author's painstaking reconstruction of land title changes. In 1636 land grants divided the future *partido* between Rodrigo Ponce de León and the Jesuit Order; by 1880 Marcos Paz had 87 proprietors. Through a combination of well-drawn cadastral maps and accompanying charts,

Moline de Berardoni summarizes vividly what took place over the intervening years. Equally valuable is her narrative description of the transfers themselves and the fate of individual owners.

The author has pursued the titles through complex and often confusing transfers, as with the fate of the Jesuit lands after the order was expelled in 1767 from Spanish America. In general, land ownership seems to have been subjected to capricious and haphazard events. Determining boundaries often produced court battles and political conflict among large landowners as nature eliminated old markers or surveyors made casual errors. Such problems sometimes inadvertently dispossessed *de facto* homesteaders from the land they occupied and worked. At times this process occurred through conscious action. Through it all, no pattern of land accumulation emerges. Instead, most would-be ranchers and *hacendados* eventually had to subdivide and sell their holdings, while others passed their estates on to relations who often mismanaged their inheritance and sold parcels to keep out of debt.

Unfortunately, the author's failure to place her work within a larger context limits its utility. For those readers familiar with Argentina's past, however, this exercise in local history should prove to be most interesting.

WILLIAM J. FLEMING  
Indiana University–Purdue University,  
Indianapolis

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## Collected Essays

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These volumes, recently received in the *AHR* office, do not lend themselves readily to unified reviews; the contents are therefore listed. Other similar volumes that are amenable to reviewing will be found in the review section.

BARBARA FINKELSTEIN, editor. *Regulated Children/Liberated Children: Education in Psychohistorical Perspective*. New York: Psychohistory Press. 1979. Pp. 225. Cloth \$15.95, paper \$6.95.

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# Communications

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*A communication will be considered only if it relates to an article or review published in this journal; publication is solely at the editors' discretion. Letters may not exceed seven hundred words for reviews and one thousand words for articles. They should be submitted in duplicate, typed double-spaced with wide margins, and headed "To the Editor."*

TO THE EDITOR:

I would like to make a few remarks about James A. Henretta's article, "Social History as Lived and Written" (*AHR*, 84 [1979]: 1293–1322), and particularly about his references to French methodology, which were not, I think, fully appraised in the comments of either Darrett B. Rutman or Robert F. Berkhofer, Jr. Nobody could seriously deny the need for a little more philosophizing within social history's rank and file. Therefore, what a pity it is that one of the rare attempts to satisfy this need should misrepresent thoughts from a country that has probably contributed, in the last two generations, more ideas about the nature of social history than any other.

Here are what I find to be the most misleading parts of the article. First, it is difficult to see how the *Annalistes*, by whom I presume is meant the contributors to *Annales: Économies, sociétés, civilisations*, represent a "coherent intellectual system." The United States has no monopoly on intellectual anarchy where historical writing is concerned. There is no one single method, no one theory, no one ideology that dominates the pages of this journal. In fact, it is interesting to note how great an influence English-speaking historians have on *Annales*, in terms both of works cited and of articles contributed. The variety of works published in *Annales* surely belies the existence of an *Annales* school.

Second, many would be surprised to see Fernand Braudel's historical world classified as a variety of French structuralism (*à la* Claude Lévi-Strauss); while I think I understand Professor Henretta's reasoning, I believe it is deceptive. The chronology implied in the article is wrong. Braudel's *La Méditerranée* was first published in 1949. Ferdinand de

Saussure's linguistics, it is true, were formulated at the beginning of this century, but they only became current knowledge in the 1950s. Likewise, the full impact of Lévi-Strauss's structural anthropology was not felt until the late 1950s and early 1960s. It is not possible that Braudel "proceeded to apply this analytic framework to the history of the vast Mediterranean" (p. 1298)—the Mediterranean had already been braudelized. Analogies between Braudel and Saussure or Lévi-Strauss are superficial; an approximation of terminology does not prove an approximation in epistemology. Furthermore, the famous three-part schema that shapes *La Méditerranée* is neither the only nor even the predominant perception of time found in *Annales* today. Perhaps a discussion of Bergson's flux of time would have been more pertinent. But, again, there is no single school of thought here.

Third, Saussure's opposition between *langue* and *parole* is no longer seriously entertained by linguists today. Specifically, the sort of social contract that the idea of *langue* represents (whereby all members of a linguistic community submit themselves, en bloc, to an unchangeable, *static* framework of rules) has given way to the more contemporary appreciation of linguistic competence (in which memory becomes a *functioning* system of rules rather than an idle depository of signs). Linguists seek to establish a system of rules from which an infinite number of phrases can be generated—the creative capacity of language knows no limits. Thus, if language be its model, there can be no basis whatsoever for Professor Henretta's claim, repeated almost verbatim by his commentators, that French structuralism is "static" and "pessimistic."

Fourth, Professor Henretta was right in setting Michel Foucault up against nineteenth-century notions of unilineal causation, but he was mistaken in citing him as an example of structuralist thinking—at least, if he is to be consistent. Once more, Henretta has confused terminology with epistemology; Michel Foucault comes from an entirely different tradition. If Henretta really intended to demonstrate that "the concern with power and change



clearly differentiates the Marxist analytic framework from that of the later *Annalistes*, with its static assumptions, its causal diffuseness, and apolitical stance" (pp. 1305–06), he should have steered clear of Foucault. Foucault's works on asylums, prisons, and sexuality demonstrate a central concern with the relationship between knowledge and power or, if you will, the problem of the political effects of scientific discourse. The vocabulary might be lacking, but Foucault himself has recently shown that this was also a major concern of his *Archeology of Knowledge* (see his "Verité et pouvoir," *L'Arc*, 70 [1978]). It is also worthy of note that Foucault has most vehemently denied ever having been a "structuralist."

Fifth, I find Professor Henretta's proposed fusion of Marxist phenomenology with the "objective" structuralist viewpoint, along with the opposition he made earlier in his article between "rigorous statistical proof" and "individual motivation," quite admirable. But he has opened himself up to the charge from some sectors—I am thinking primarily of those "more materialist-oriented Marxist scholars like Louis Althusser"—of subjectivism and its various ideological escorts. Personally, this would not bother me one bit, but I fear Henretta will get short shrift from those scholars of high moral purpose whom he so obviously admires.

All of the problems I have indicated, save the last, are the result of the widespread tendency among historians to throw, when they scent the slightest similarity, batches of different ideas, methods, and philosophies into the same bread-basket. Perhaps this tendency results from the universities' demand for survey courses of ludicrous breadth, their lack of language requirements, or the publishers' demands for books of extraordinary scope. I am wholeheartedly sympathetic to Professor Henretta's call for more thought in this new, exciting field of history. But I am afraid we are going to be hearing about the static, pessimistic *mentalité* of the *Annalistes* for some time to come. It is a shame.

GEORGE DALLAS  
Princeton, New Jersey

#### TO THE EDITOR:

I have just received a copy of the review of my book *The Defence of British Trade, 1689–1815* (*AHR*, 83 [1978]: 1006) via my publishers and should like to reply. The review contains a great many errors of fact and understanding, and a detailed response is called for.

In the final paragraph George F. Steckley stated, "A map of the Baltic contains none of the place names mentioned in the text." But page 74 names Riga, Stockholm, Danzig, and Gothenburg, all of which appear on the map. I accept, however, that "Tonnage" in the title to Table 6.2 should read

"Number." "Market value" is no more accurate a term than "real value"; as the market varied, so did the price. "Commerce" is never used as an adjective; at times it is used as a compound noun—that is, commerce defence. My sentence construction is grammatically correct, and no other reviewer has found the book hard to understand. On this basis, I cannot accept any of Steckley's final paragraph except for one small point.

Turning to the opening paragraph, Professor Steckley apparently believed that my book covers the seventeenth century. In fact, it includes no more than the last eleven years of it. His remarks about piracy thus have little relevance, since they refer to the period before my book starts. The last remaining pirates were rounded up in the final years of the seventeenth century, as I noted on page 176. Steckley repeated the old myth about "Barbary pirates." In fact, they were privateers—that is, private warships licensed by the state. There is a fundamental difference; pirates by comparison acted outside the law and were hanged when caught. Steckley's assumption that England's trade with the countries bordering on the Mediterranean was under effective control of the Barbary corsairs is also wrong. England possessed a strong navy and defended its commercial interests; other nations were less fortunate.

In the second paragraph Professor Steckley stated that "a chapter on insurance sketches the early history of Lloyd's." In fact, the chapter is about *marine* insurance and describes in some detail the development of English marine insurance. Lloyds is misspelled; equally important, the chapter describes the way in which the terms of the policy came to be defined in law. Nor did I "suggest that merchants, insurers, and the Admiralty cooperated in enforcing convoy discipline." On page 93 I *stated* that they did.

I do not accept Professor Steckley's remarks in the fourth paragraph, and his opening remark is unfounded. The argument hinges on being able to establish accurate records of shipping losses and their causes. No records exist, and they would be of doubtful value if they did. Contemporary lists of convoys outward bound do not record whether they arrived safely. In any case, tonnage figures usually varied from voyage to voyage. Nor has Steckley appreciated that "losses" include vessels captured by enemy privateers and warships as well as those sunk at sea from a variety of causes. My approach has been to take estimates of total trade and show how effective naval defense was in each war. Similarly, Steckley's remark in the following paragraph—"Crowhurst offers no conclusion about the efficiency of the various defensive measures"—I reject on the grounds just outlined.

Finally, I should like to comment on Professor

Steckley's suggestion that East Indiamen or vessels of a similar type—that is, built like naval frigates—would have been more suitable to carry the produce of the tobacco and sugar trades. In the first place, the East India Company needed the ships for its own trade and would have kept them; it had enormous political power. If Steckley has assumed that similar ships could have been built at short notice, he has not considered the cost of ship-building in wartime or of the time needed to build large ships when materials were scarce. Also, Steckley has not considered who would have had to pay for them or the political furor such a proposal would have caused. As I have demonstrated in my book, governments wanted allies among the merchants during wartime, not enemies. The huge financial demands forced governments to raise money by loans, and for this they had to have good relations with merchants and commercial companies.

Professor Steckley's comments are thus based on a misunderstanding of much of the subject matter and on a hasty reading of my book. Other reviewers have been more perceptive, and I have received a very favorable press on this side of the Atlantic.

PATRICK CROWHURST  
*Leicester Polytechnic*

PROFESSOR STECKLEY REPLIES:

I welcome this opportunity to reply to Patrick Crowhurst's comments on my review of his book, and I will do so in the order of his criticisms.

I should have thought that the map of the Baltic was included on page 51 to help the reader locate places named in the discussion of the relevant place names found on page 52, not on page 74. But none of the relevant place names on page 52—the Sound, Heligoland, Elsinore, and Hamburg—is found on the map.

Professor Crowhurst's comments on real value confirm my earlier suspicion that he does not understand the meaning of the term. He stated that the "real value of commodities imported by [the East India Company] had declined" (p. 212), but he offered no evidence that he deflated the market values of those goods with an appropriate price index—that is, he failed to show that East India goods per unit were exchanging for less in terms of other goods. What I think he intended to say is that the nominal market prices of Eastern goods were falling below the "set of book values" (p. 213) used by the Customs officers in assessing the trade. And this has nothing whatsoever to do with a change in real value.

I stumbled frequently over such compound nouns as "commerce defence," "commerce war," and "commerce raiding." I would have preferred more conventional forms. But surely no one should

be asked to read sentences like the following: "By the close of the seventeenth century Massachusetts and New York was paying for its English imports by exports to the Caribbean" (p. 108); "There was also difficulties of controlling such a large body of ships at sea" (p. 148); and "Many were attracted by the opportunity for a little private trade, some were obtained by crimps and entered the service unwillingly, but the voyages though long held no more hazards than the Caribbean trade and was far healthier than the African" (p. 218). There are others. But I must admit that I enjoyed trying to imagine the event that the following sentences describe: "The French made a number of plans to capture [St. Helena] but none succeeded. During the Seven Years War d'Estaing and Frogier de l'Eguille had both proposed the capture of the island, and in the War of American Independence Kerguelin planned another attempt—it was captured on board the *Libre Navigateur*" (p. 246). I believe that I was giving the author the benefit of the doubt by supposing in my review that there had been a failure to edit the text.

Nothing in my review suggests that the book "covers the seventeenth century." I merely noted in my introductory paragraph that the English in an earlier period had found means other than convoying to protect merchant ships against some hostile powers. And, if Professor Crowhurst is right in maintaining that all Barbary corsairs were privateers licensed by the state, he has strengthened my point.

There seems to be an honest disagreement among distinguished British authors about how one ought to spell "Lloyd's." I have followed, among others, Ralph Davis, whose excellent studies of eighteenth-century trade Professor Crowhurst has frequently cited, and D. E. W. Gibb, historian of Lloyd's and himself a member of the firm.

Nothing in my review suggests that heavily armed merchant ships should have been built "at short notice . . . in wartime" to serve the "sugar and tobacco trades." I have merely asked why some ships of such design, like those that the East India Company found the resources to build during peacetime, might not have been built for use in various trades in order to reduce the need for convoys and to shift the cost of protection from the taxpayer to the consumer.

I am not the only reviewer, I have recently discovered, who has noted that Professor Crowhurst's method is one of assertion rather than demonstration and that his book lacks comparative generalization. And, because Crowhurst has failed even to estimate the costs of providing convoy, he cannot offer useful conclusions about the efficiency of that service.

Finally, I can assure Professor Crowhurst that I

did not give his book "a hasty reading." I felt a responsibility as a reviewer to consider his arguments as carefully as I could; and, even if I had been reading the book for pleasure alone, I would have been unable, for the reasons given, to read it with haste.

GEORGE F. STECKLEY  
Knox College

TO THE EDITOR:

Paula S. Fass is so far off in her imaginings (*AHR*, 84 [1979]: 1465–66) that I wrote my *Vanguards & Followers: Youth in the American Tradition* with "evident disgust," with "alarm and fear," "anger," "pain," and "distant disdain for trivial youth" as to require comment. Please note that these fantasies about my feelings—there is a prefatory statement she could have quoted—obviously relate to my account of youth in the 1960s and early 1970s. I cannot understand how she could try to relate "evident disgust" and the like to my chapters on the larger part of the youth *tradition*, involving as they do leaders and developments (including "Young America") and going back to American beginnings.

My book, according to Professor Fass, is not history but a "personal testament." It lacks methodology. It emphasizes individuals. Is this true, also, of my *Randolph Bourne*, which deals with the first modern youth crystallizations, from which later ones derived? There is no evidence that Fass knows this book. In her own research about 1920s youth, she thinks *conformist* youth of the time, defined precisely as "native, white, middle-class, and almost exclusively college-going" (p. 7) writing for college journals, is the true youth. She manifestly has no idea of the role of *visible and effective* youth of any given decade. She would deem Carlyle no historian. I assert, without derogation, that youthful fads and vagaries such as she expanded upon do not determine the career of a Hitler, a Mussolini, a "Che," and their regiments of young followers—or, at home, a young Garrison, a young Bellamy, a young Upton Sinclair (as distinguished from a young John Reed), and others who built traditions long before the 1920s, and the grand pay-off of the 1960s—all of whom I have treated in writings to which Fass made no reference.

I reduce, she claimed, culture to personality, as if, for example, Norman Mailer's mayoralty challenge did not determine a New York election or, before him, Walt Whitman's or Ralph Waldo Emerson's explicit appeal to youth was "trivial." Were they trivial, or did they motivate vital segments of youth?

The basic wrongness, I repeat, of her unsubstantiated charges, which do not cope with any of the above, passes over the fact that nowhere in *Vanguards & Followers* did I separate the youth tradition

from the necessary aid and cooperation of elders. Youth was not age. Youth was a point of view. I cannot here recap my *evidence* as set down for the several eras involved, and for the book as a whole. It is displayed at length in my *Randolph Bourne* and, indeed, elsewhere, for those who are interested. It suffices that the reviewer cites no proof of any kind for her assertions.

Most glaring, most expressive of unstated emotion, is her stated hope that "serious historians" will not "include [my] book within the growing literature on youth today." I must curb many thoughts that sentence suggests. It must suffice that this forthright appeal to repression in the history field—even to refute—does demand, in so basic a publication as the *AHR*, that she give us some of the names and titles of the "many historians" not included in my 20-page bibliography who have been investigating the larger picture of youth in America, whose methodology she approves. Others beside myself will await her response and her list.

LOUIS FILLER  
Antioch College

PROFESSOR FASS REPLIES:

I gladly offer the following books and articles as examples of the range of currently available work useful for the study of "youth in history." (1) Philippe Ariès, *Centuries of Childhood: A Social History of Family Life* (1962); (2) Robert H. Bremner et al., eds., *Children and Youth in America: A Documentary History*, 3 volumes (1974); (3) Natalie Z. Davis, "The Reasons of Misrule: Youth Groups and Charivaris in Sixteenth-Century France," *Past & Present*, no. 50 (1971); (4) S. N. Eisenstadt, *From Generation to Generation: Age Groups and Social Structure* (1956); (5) John R. Gillis, *Youth and History: Tradition and Change in European Age Relations, 1770–Present* (1974); (6) Oscar Handlin and Mary Handlin, *Facing Life: Youth and Family in American History* (1971); (7) Joseph F. Kett, *Rites of Passage: Adolescence in America, 1790 to the Present* (1977); (8) Gilman M. Ostrander, *American Civilization in the First Machine Age, 1890–1940* (1970); and (9) Steven Schlossman, *Love and the American Delinquent* (1977).

"Youth today," which is Louis Filler's misquotation from my review, is a quite different subject and one about which neither I nor Professor Filler are expert.

PAULA S. FASS  
University of California,  
Berkeley

TO THE EDITOR:

I would like to respond to several issues that Richard J. Evans raised in his review of my book, *The*

*Jewish Feminist Movement in Germany* (AHR, 85 [1980]: 417–18). Evans believed I “strained” to give the Jüdischer Frauenbund (JFB) “feminist credentials.” He quoted from a section entitled “the contradictions of Jewish feminism . . .” in which I explained the constraints on the feminist leadership by a more conservative following, by their own bourgeois upbringing, and by their fears of antisemitism. It would be equally facile to pick out the “feminist” quotations, but the goals of the JFB speak for themselves. These included equal education and career opportunities for women, support of the goals of the German women’s movement, suffrage in the Jewish community, equal pay for equal work, the care of unwed mothers, and the elimination of discrimination against women in Jewish law and the elevation of women’s image in general. Theirs was a “social feminism” (p. 13)—practical social reforms to improve the circumstances of women’s lives—similar to the social feminism described in such historical works as J. Stanley Lemons’s *The Woman Citizen: Social Feminism in the 1920s* (1973) or William O’Neill’s *Everyone Was Brave* (1969). While justifying itself in reformist language, the JFB went well beyond “a comfortable resemblance to traditional Jewish charities” to attack male hierarchy and provide woman-oriented alternatives. Its ideas, accomplishments, and failures must be understood within the context of German history: the weakness of German liberalism, the omnipresence of antisemitism, and the intransigence of antifeminism. In any case, “feminism” ought not to be narrowly defined. We should look for women’s achievement and appraise their lives in a far broader context. An understanding of the variety of ideas and methods with which women tried to gain more control over the making of their own history suggests that feminism is a *process* rather than an “ideological commitment.” While recognizable over time, it assumes different forms and expresses different goals specific to its historical situation. Many women knew well enough to pursue their own interests without Evans’s “most minimal definition.”

Evans wrote that I presented no evidence that the JFB “ever demanded anything more than women’s suffrage on local and Jewish welfare bodies.” He has failed to understand the importance of these bodies to Jews and the radical nature of the JFB’s demands within this religious context. The *Gemeinde* was a publicly constituted corporation, automatically embracing all Jews in any one place and empowered by the government to tax its members. One could take the extraordinary step of seceding from the *Gemeinde*, but few Jews did. The decisions of this corporate community regarding cultural and religious life and social welfare policies had a profound impact on the lives of all Jews. In condemning JFB members for not having done much

for “wider feminist goals” in Germany, Evans has denied the legitimacy of their primary religious and ethnic identification and disregarded their perceived and actual rejection by German society.

Evans suggested that I “missed an important opportunity to explore the psychological implications” of the fact that the leader of the JFB campaign against white slavery was Bertha Pappenheim, Freud’s “Anna O.” Although it is tempting to speculate about the personal and psychological motivations of any purity reformer, the important point is that white slavery was a major problem that troubled Jewish communities in Eastern and Western Europe and in the Americas. White slavery was not the figment of someone’s repressed sexual fantasy.

I nowhere portrayed the JFB as the “most important religious organization” in the German women’s movement. What I said was that “the JFB was the only religious women’s organization in Germany to promote feminism in its goals *and* to belong to the German women’s movement.” Jewish feminists were painfully aware that the right-wing Protestant Women’s League carried far more weight within the national organization, despite the JFB’s longer loyalty, larger membership, and more progressive complexion.

Finally, my book is based on the JFB’s periodical—about 15 percent of the footnotes refer to it—as well as on secondary literature, archival materials, memoirs, interviews, and other primary sources, including a variety of publications by Pappenheim and the JFB.

MARION A. KAPLAN  
New York City

#### PROFESSOR EVANS REPLIES:

Marion A. Kaplan claims that “the goals of the JFB speak for themselves.” But the goals of the JFB as listed on pages 86–87 of her book are not the same as those claimed in her communication. Equal education and career opportunities for women and equal pay for equal work were supported by the JFB only insofar as they were (like votes for women) on the program of the umbrella organization of the German women’s movement, the BDF, to which the JFB belonged. The BDF program was not binding on member associations, and many of them did not support many of the points on it or paid only lip-service to them. The German Association against the Abuse of Alcoholic Drink, for instance, belonged to the BDF; it was in fact the largest member association (not the JFB, as Kaplan claimed); but it can hardly be regarded as an active feminist association; it only belonged in order to secure the BDF’s support for the temperance cause. Membership in the BDF did not, in itself, make an



organization feminist; the antifeminist Protestant Women's League, for example, also belonged. Nor are the other JFB goals that Kaplan listed necessarily feminist. The Catholic Church had homes for unwed mothers—did that make it feminist? The Nazi Party campaigned for what it understood as the elevation of women's image in general—did that make it feminist? On many more accurate touchstones of feminist commitment, the JFB must be found wanting. The discussion of the JFB's attitude to careers for women on pages 171–72 of the book fails to show that the JFB ever demanded women's entry into the higher professions, for instance. And on page 187 Dr. Kaplan even remarked that “Jewish feminists almost never demanded for themselves the right to work outside the home.”

Certainly the JFB was feminist, if we accept Dr. Kaplan's definition of feminism as the process of women's pursuit of their own interests, or their attempts to gain more control over the making of their own history. But this definition is so wide as to be virtually useless in historical analysis. Since the act of organizing under their own leadership, for whatever purpose, constitutes *ipso facto* an attempt by women to gain more control over their own lives, and is bound by them to be regarded as being in pursuit of their own interests, any women's organization will be a feminist organization on Kaplan's criteria. Does she want to suggest that women's antisuffrage leagues were feminist? Suprahistorical definitions of this kind simply will not do. Nor do I think O'Neill's concept of “social feminism” is very helpful. Some consideration of ideology is necessary. Judged by the standards of their own time, and taken at their own estimation, the women of the JFB were not feminist, and the evidence that Kaplan presented for this view in her book far outweighs any evidence on the other side.

Dr. Kaplan's second paragraph is a grotesque and insulting misreading of what I wrote in my review. I fail to see how any rational observer could interpret my review as denying the legitimacy of the JFB members' primary religious and ethnic identification, disregarding their rejection by German society, failing to understand the importance to them of welfare bodies, or belittling the significance of their demands within the Jewish community. In my review, indeed, I went out of my way to praise Kaplan's handling of the problems that Jewish women encountered within the Jewish community and in German society as a whole. Where I did—and do—think the book is misleading is in its attempt to imply that the JFB actively pursued wider feminist goals advocated by leading feminist organizations of the day, such as the parliamentary suffrage. Kaplan resorts to the wild and fantastic allegations that constitute the second paragraph of her communication because she is unable to provide a

reasoned reply to the criticisms that I made of her book in this respect.

Similarly, I fail to see how on earth my review could be taken to imply that “white slavery” was “the figment of someone's repressed sexual fantasy.” Once more, Dr. Kaplan is reading into the review something that just is not there. Like all social reform movements, the crusade against “white slavery” arose out of the identification of a real social problem, but it was also conditioned by the motives and attitudes of those engaged in it. One needs to explain why Pappenheim devoted her energies to this rather than to other causes, and Kaplan did not, in my view, explore the personal dimension of this choice satisfactorily. Kaplan herself admitted on page 133 of her book that the JFB exaggerated the dimensions of the “white slavery” problem in the 1920s, but she did not ask why, nor did she consider the possibility that it might have done the same earlier.

Dr. Kaplan's denial that she portrayed the JFB as the most important religious organization in the German women's movement is thoroughly disingenuous. Her quotation continues: “The Protestant Federation of Women, begun in 1899, supported ecclesiastical suffrage, but did not approve of women's suffrage in the nation and withdrew from the BDF shortly after joining it” (p. 152). Here Kaplan seemed to me to be making parliamentary suffrage the criterion of feminist commitment. Yet the JFB gave active support only to the equivalent within the Jewish community of ecclesiastical suffrage within the Protestant communion. Indeed, on the definition of feminism that Kaplan advances above, I cannot see how it can be denied that the Protestant Women's League, which played within the Protestant community a comparable role to that played by the JFB in the Jewish, was a feminist organization. But the crucial point in the text on page 152 is the claim that the Protestant women withdrew from the BDF shortly after joining it and so, by implication, played no significant role in it. In fact, they belonged to the BDF for a whole decade, from 1908 to 1918. I am glad that Kaplan now concedes that Jewish feminists were aware that the Protestant Women's League carried more weight within the BDF than did the JFB, a point she nowhere made in her book.

Finally, Dr. Kaplan's claim that only 15 percent of her footnotes refer to the JFB's periodical, though true, is equally disingenuous. A large proportion of the notes refer to comparative and secondary material. In every chapter, references to the JFB's periodical outnumber those to archival material. In all, there are over one hundred footnotes referring to the periodical and fewer than seventy referring to unpublished sources. Moreover, the overwhelming majority of the archival material comes from one



institution, the Leo Baeck Institute. The total unpublished source material consulted in Germany amounts to a single file in a single archive.

RICHARD J. EVANS  
Columbia University

TO THE EDITOR:

Richard N. Current in his review of my book, *The Day of the Carpetbagger: Republican Reconstruction in Mississippi* (AHR, 85 [1980]: 465), has distorted my treatment of Reconstruction politics. He claimed that I follow "the conservative line of that time," and he even suggested that I justified Ku Klux Klan and Democratic violence. Nothing could be further from the truth. My purpose, which other reviewers believe I achieved (see *Reviews in American History*, 8 [1980]: 69-73, and *History: Review of New Books*, 8 [1979]: 27), was to write a balanced account of postwar affairs without resorting to either old or new stereotypes. For example, I avoided labeling all conservatives as reactionaries or all Radical Republicans as honest crusaders for right and justice, as Current apparently prefers. The reviewer's charge that I used conservative sources uncritically and even adopted the comments of "reactionaries" as my own is untrue. In quoting and paraphrasing conservative spokesmen, who were frequently divided on issues, I attempted to demonstrate how they perceived Reconstruction events and developments, which to a great extent explains why they reacted the way they did to the Republican ascendancy and why the new order ultimately failed. In discussing the Ku Klux Klan, my purpose was to explain it in a broader context than has been previously done and also to indicate why the Klan achieved only a partial success before the federal government stepped in to suppress it in 1871. At no point did I justify the Klan's existence or suggest that Republican mistakes or policies justified white violence and intimidation.

Professor Current has also charged me with slighting or ignoring Republican sources. Again, he is incorrect. I made extensive use of such Republican sources as the *Jackson Pilot*, the *Vicksburg Times and Republican*, the official correspondence of the governors, the printed reports of Republican officials, Republican addresses, and Adelbert Ames's correspondence with his wife. I did not cite extensively, as Current said I should have, the congressional testimony regarding the 1875 campaign because more immediate and informative Republican, as well as Democratic, materials existed. I might also point out that Republican statements, like those of the Democrats, could be self-serving.

Professor Current was disturbed by my criticism of carpetbagger Ames, contrasting my treatment of him with my alleged failure to condemn the Demo-

cratic leadership. Since Ames and the Republicans governed the state during Reconstruction, I naturally emphasized their policies and performance, but I did mention the racism and even the hypocrisy of many Democratic leaders, although perhaps not as frequently or as strongly as Current desired. Current seemingly ignored my generally sympathetic treatment of such moderate and anti-Ames Republicans as Governors Alcorn and Powers, U.S. Senators Revels and Pease, James Lynch, and others of both races.

My criticism of Ames, which was not unqualified, was based to a great extent on non-Democratic sources. Many Republicans, including national leaders, expressed dismay at Ames's efforts in 1869 to build up a political following in Mississippi while serving as the military district commander. As I indicated in the book, General Ames temporarily "opened wide the door for adventurers and scamps," and in early 1870, while still in command, high-handedly secured his own election to the U.S. Senate. But, in the same sentence in which I charged Ames with playing "the role of military despot," I commended him for his "vigorous efforts to maintain the social equilibrium in the state and prevent attacks on Republicans" (pp. 253-54).

Contrary to what Professor Current said, I have not placed the sole blame on Ames and his followers for the failure of Reconstruction. Finally, Current's concluding comment that Harris "does subscribe to the principle of racial equality—as did the Mississippi conservatives in the 1875 platform" is gratuitous, to say the least.

WILLIAM C. HARRIS  
North Carolina State University

PROFESSOR CURRENT REPLIES:

Nothing in William C. Harris's letter persuades me to change my opinion of his book or my review of it. His letter does, however, provide an opportunity for me to confirm and, perhaps, to clarify my criticism. My review describes his work on Mississippi Reconstruction as "prodigious," one dictionary definition of which is "extraordinary in size, amount, extent, degree, force, etc." The review also refers to it as the most "circumstantial" account of Reconstruction in any state, "circumstantial" meaning in this context "dealing with or giving circumstances or details." These characterizations certainly imply that the study is valuable if not indispensable for any scholar interested in the subject. The review also mentions other strengths of the book and then, to caution users of it, goes on to indicate the author's bias. Harris obviously thinks he has none, but any careful reader who is familiar with the Mississippi politics of the period will see that, in important respects, he did indeed follow "the conservative line of

that time." When, for example, he said the Ku Klux Klan directed its activities not against black schools as such but only against "obnoxious" teachers, he was clearly following the line and he was also mitigating if not justifying Klan atrocities. If he quoted and paraphrased conservatives "to demonstrate how they perceived Reconstruction events," he neglected to give the reader any hint as to which views were exclusively theirs and which are also his own.

As for his "extensive use" of Republican sources, the real question is the kind of use Professor Harris made of them, which was essentially to corroborate conservative charges rather than to present a case for Ames and the Ames Republicans. Not that all of the Radicals were at all times simply "honest crusaders for right and justice"—a judgment that Harris falsely imputes to me. But, after all, Ames as

governor was trying to advance the cause of Reconstruction, including the assurance of political rights and social justice for blacks. The conservatives, Alcorn along with them, were doing all that sedition and terrorism could do to frustrate that cause. Hence, it seems like rather strange logic for Harris now to explain that, since "Ames and the Republicans governed the state," he has "naturally emphasized their policies and performance" in accounting for the failure of Reconstruction in Mississippi. Finally, there is nothing "gratuitous" in the concluding sentence of my review; it is intended to clinch the point of the like-mindedness (apparently quite unconscious on his part) between Harris and the Mississippi conservatives of 1875.

RICHARD N. CURRENT  
*University of North Carolina,  
Greensboro*

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## Recent Deaths

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SIR GEORGE NORMAN CLARK, formerly Regius professor of modern history at Cambridge University and provost of Oriel College, Oxford University, died on February 6, 1979, in Oxford. Born on February 27, 1890, Clark was educated at Bootham School and Manchester Grammar School before going up to Balliol College, Oxford, where he was a Brackenbury Scholar in 1908. He took a First in Literae Humaniores in 1911 followed by a First in modern history in 1912, the same year in which he became a fellow of All Souls. Following service in France during World War I, he returned to Oxford to become a fellow and lecturer at Oriel College from 1919 to 1931. During those years he held a succession of college and university positions, becoming tutor at Oriel in 1922 and librarian in 1930, as well as being university lecturer in modern history 1927–31 and university proctor 1929–30.

Already in these years he had established himself as a formidable and original scholar. His *Dutch Alliance and the War against French Trade* (1923) was instantly recognized as a significant contribution, while his outstanding talents as an editor were revealed in *The Churchwardens' Accounts of Marston* (1925) on which he collaborated with F. W. Weaver. Those same talents were put to the service of the *English Historical Review*, the staff of which he joined in 1919; he became editor of that journal in 1920, holding the post until 1925 and then serving a further year as joint editor. He was ultimately to have the unusual distinction of serving as editor of the *English Historical Review* on two separate occasions, for he resumed the editorship for a brief period in 1938, paving the way on that occasion for the editorships of Goronwy Edwards and Richard Pares. Perhaps the most noteworthy product of these years was his remarkable *Seventeenth Century* (1929), a highly original synthesis of material at the time of its publication and still an inspiring work more than half a century later.

In 1931 Clark was elected Chichele Professor of Economic History and fellow of All Souls, positions he held with distinction until 1943 when he moved to Cambridge University as Regius Professor of

Modern History and fellow of Trinity College. He returned to Oxford as provost of Oriel College in 1947 and presided there for a decade, retiring in 1957. From 1961 to 1975 he was again a fellow at All Souls.

Numerous academic distinctions were awarded to Clark, including honorary degrees from a host of institutions (Aberdeen, Utrecht, Durham, Sheffield, Hull, Columbia, Dublin, and Cambridge), honorary fellowships at Trinity College, Dublin, Trinity College, Cambridge, and Balliol, Oriel, and All Souls Colleges, Oxford, fellowship in the British Academy, of which he was president 1954–58, and a knighthood in 1953.

Throughout his career, Clark was a prolific publisher, making signal contributions both to English history and to Dutch history, in which field he continued to contribute reviews to the *English Historical Review* for a number of years after his retirement. His guiding hand was vital to two of the ambitious cooperative projects of his time, the *Oxford History of England* (to which he contributed the well-known volume, *The Later Stuarts* [1934]) and the *New Cambridge Modern History*, the planning of which coincided with his Cambridge professorship. The range of his scholarship is well indicated by the titles of his most significant volumes in addition to those already mentioned: *Science and Social Welfare in the Age of Newton* (1937); *Guide to English Commercial Statistics, 1696–1782* (1938); *The Colonial Conferences between England and the Netherlands*, with W. J. M. van Eysinga, two volumes (1940–51); *The Wealth of England* (1946); *Early Modern Europe* (1957); *War and Society in the Seventeenth Century* (1958); and *History of the Royal College of Physicians*, two volumes (1964–66). In addition he held a number of distinguished lectureships, being Creighton Lecturer at London (1948), Ford Lecturer at Oxford (1949–50), Murray Lecturer at Glasgow (1952), Wiles Lecturer at Queen's University, Belfast (1956), Donnellan Lecturer at Trinity College, Dublin (1960), Whidden Lecturer at McMaster (1960), and Leslie Stephen Lecturer at Cambridge (1965).

Sir G. N. Clark established himself throughout a

long and productive life among the masters of early modern history. Thorough and meticulous in scholarship, clear and penetrating in expression, broad and original in the conception of his themes, he has left behind a legacy of scholarship of first-rate dimensions.

ROGER HOWELL, JR.  
*Bowdoin College*

CEDRIC C. CUMMINS died April 12, 1980, in a hotel fire in Madison, South Dakota, where he was attending the annual Dakota History Conference.

Professor Cummins was born in English, Indiana, on March 14, 1909. He completed his Ph.D. work in 1944 at Indiana University where he was a student of R. Carlyle Buley. He was professor of history at the University of South Dakota from 1946 until his retirement in 1976. He was department chairman for fifteen years. His major publications include *Indiana Public Opinion and the World War, 1914-1917* and *The University of South Dakota, 1862-1966*.

It is perhaps as a teacher and friend that he will be best remembered. His kindness and thoughtfulness were his hallmark to both students and colleagues. His patience and gentle manner were always present, whether in the field hunting pheasants, or trying to capture a wildlife scene on film, or in a raucous department meeting to discuss the development of a Ph.D. program. He was at peace with himself and with his fellow man, a state of tranquillity to which we all aspire, often with lesser degrees of success.

R. ALTON LEE  
*University of South Dakota*

AMY M. GILBERT, professor emeritus of history at State University of New York, Binghamton, died in Endicott, N.Y., on February 27, 1980, after a long illness. She was born in Chambersburg, Pennsylvania, on February 23, 1895. After attending Wilson College, from which she received her A.B. in 1915, she went on to the University of Pennsylvania, where she earned her M.A. in 1919 and her Ph.D. in 1922. She was awarded an LL.D. by Wilson College in 1939, in recognition of her academic achievements. While still a graduate student, she taught at Clinton (N.J.) High School and Wilson College. Upon receiving her Ph.D. she accepted a position at Elmira College, where she rose to the rank of professor and served as head of the history department. She left Elmira College in 1936 to become academic dean at Milwaukee-Downer College. Five years later she was appointed dean of women at Rhode

Island College, where she remained until 1945. She served as visiting professor at Temple University during 1945-46. In 1946 she was named head of the departments of history and political science of the Associated Colleges of Upper New York (ACUNY)—comprising Champlain College, Mohawk College, Sampson College, and Middletown Collegiate Center—which had just been established as a temporary institution designed to meet the postwar demands on higher education. After the termination of ACUNY, in 1950, and the transformation of Champlain College into the first liberal arts unit of State University of New York, she was named the college's dean of academic instruction. Two years later, when the existence of the college was threatened by the U.S. Air Force's declared intention of taking over the land on which the college was located, she was appointed head of the college. After all efforts to save the college had failed and it was closed, in 1953, she was invited to teach at Harpur College, which was to become the liberal arts unit of State University of New York at Binghamton. She taught European history there until her retirement as professor emeritus in 1965.

Professor Gilbert was a remarkably dedicated person, of exceptional strength of character and firmness of purpose, who never forgot that institutions exist to serve people. In the course of her long career she performed outstanding work in many roles. As an administrator she faced and successfully dealt with many difficult challenges. This was particularly true of the ACUNY years, when she dealt magnificently with her broad and demanding responsibilities in that fledgling and amorphous institution. Then followed two years of pioneering work in setting Champlain College on its course, succeeded by a year at the thankless task of administering a college under the threat of extinction. As a teacher she acquired a loyal following among her thousands of students, who appreciated her devotion to learning, her breadth of intellectual interest, her commitment to intellectual discipline, her concern for them as human beings.

Professor Gilbert's first book was *The Work of Lord Brougham for Education in England* (1922). She contributed a chapter on the history of the woman's movement to *The History of New York State* (1933-37), edited by A. C. Flick. Her *ACUNY: The Associated Colleges Of Upper New York* (1950) is an official history that ably records an ephemeral yet important chapter in the history of higher education. After her retirement, she was able to complete a work on which she had long been engaged, *Executive Agreements and Treaties, 1946-1973* (1973), a work that reflected her continuing interest in international law. This was the first book on the subject since Wallace McClure's study published a generation earlier.

The many friends Professor Gilbert made during

her extended career will long remember her with respect and affection.

SIDNEY HARCAVE  
*State University of New York,  
Binghamton*

RAPHAEL N. HAMILTON, S.J. died in Milwaukee on April 19, 1980, after almost fifty years at Marquette University as a historian and archivist. Born in Omaha, Nebraska, in 1892, he was educated there and received his A.B. from Creighton University in 1913. That winter he entered the Society of Jesus. His graduate training was completed at St. Louis University. There he received a master's degree in history in 1919 and his clerical training, which culminated in his ordination in 1926. Finally in 1931 he received a Ph.D. in American history.

In 1930 he came to Marquette University as a member in the history department and in 1932 received the appointment as professor and head of the department. From then until he was retired as emeritus professor in 1964, he directed the department until 1957 and taught a wide range of courses. He was an excellent teacher and, in particular, carefully guided a considerable number of M.A. students who later taught history in Milwaukee schools and in Catholic high schools throughout the Midwest.

Fr. Hamilton joined the American Historical Association in the 1920s and wrote for its *Review* and other historical and religious journals. His first major monograph was a history of Marquette University (1952). Later he wrote on Marquette, the man, with a biography of Jacques Marquette (1970) in the Great Men of Michigan series. And in 1970 the University of Wisconsin Press published his *Marquette's Explorations: The Narrative Reexamined*, an investigation of the sources of Fr. Marquette's account.

A second career began for Fr. Hamilton in 1961, when he was asked to organize the Marquette archives, and from 1964 he devoted his full time to this new task. Along with the university archives, he invited other collections, and developed a body of records that documented the trend of American Catholic social changes in the late nineteenth and twentieth centuries. He retired as university archivist in 1972, but kept on working at the archives until the end of 1977.

Fr. Hamilton had a wonderful ability to help others and inspired a great number of historians and archivists. At his wake, a student who took his classes in the 1930s remarked that he still retained the vision of history that Fr. Hamilton had taught him.

ROBERT CALLEN, S.J.  
*Marquette University*

THURMAN EVERETT PHILOON was born in 1914 in Winchester, Massachusetts. As an undergraduate at Bowdoin College, he was inducted into Phi Beta Kappa as a junior, won the senior Greek prize, the Latin prize two years in a row, and graduated with Highest Honors in Classics, *summa cum laude*.

After graduation, he taught Latin, history, and English for three years in a Massachusetts high school. In graduate school, he received the M.A. from Harvard in 1941, then served in the United States Army until 1946. After World War II, he entered Yale, receiving the Ph.D. in 1950. In 1953, following three years of teaching history at the University of Bridgeport, he ventured from his native New England into Pennsylvania to join the department of history at Franklin and Marshall College.

He wore his impressive academic credentials and honors with unselfconscious grace, never overproud, never seeking to impress. Fluent in German, he could nevertheless joke about his difficulties with the language. From his sabbatical quarters in Marburg, he wrote a friend: "[Helen and I] wish we could adapt to the language as quickly as the radio we brought [with us] has."

During his past twenty-seven years at Franklin and Marshall, he taught a variety of courses covering ancient, medieval, Renaissance, and Reformation history . . . and his speciality, military history. Not long ago he introduced a new course into the curriculum, and during the past few years he was experimenting with the integration of films into his military history course.

His scholarly interests were as varied as his courses, with publications in medieval history and Reformation studies, and research interests ranging from the role of the elephant in the Greek and Roman worlds, to the role of Hessian mercenaries in the American Revolutionary War.

His students were fond of him, perhaps because he never seemed too busy to talk with them about history or their personal problems, perhaps because of his friendly, warmhearted manner. He was a veritable pipe-smoking, academic Santa Claus of a man, with a twinkle in his eye, and a wonderful down-East brogue. Through his enthusiasm, some of his students undertook independent study on military history. He would invite them along on his many research trips to the Army War College Archives in Carlisle.

A longtime member of the American Historical Association, Thurman E. Philoon died of a heart attack on May 11, 1980. A memorial service at the College on May 14 honored him.

LOUIS L. ATHEY  
*Franklin and Marshall College*

PETER ROMANOFKY, associate professor of history at Jersey City State College, died of cancer, sur-



rounded by his family at his home in New York City, on May 4, 1980. He was thirty-seven years old. A native of Long Beach, New York, Peter received his B.A. from Queens College (1964) and his M.A. (1965) and Ph.D. (1969) from the University of Missouri, Columbia, where he worked with John Lankford. He spent one year as a visiting assistant professor at Sir George Williams University before joining the faculty of Jersey City State College in 1970.

Peter was a remarkably beautiful human being. A kind and compassionate friend, he was unfailingly generous with his time and hospitality. He was a gifted teacher, supportive colleague, and thoughtful scholar whose integrity, friendship, dedication, and curiosity were never more evident than in his courageous struggle against the dreaded disease that took his life. He never gave up and lived a fuller life in his last five years than most of us do in a lifetime. A conscientious historian, Peter was a member of the American Historical Association, the Social Welfare History Group, Columbia University's Faculty Seminar on the City, and the Immigration History Society and a life member of the Organization of American Historians.

He leaves behind a rich scholarly legacy. A specialist in the history of social welfare, Peter's articles on dependent children, foundlings, and adoption have appeared in the *Missouri Historical Review*, *American Jewish Historical Quarterly*, *Bulletin of the History of Medicine*, *Jewish Social Studies*, and *New-York Historical Society Quarterly*. These essays are characteristically well documented, meticulously crafted, and gracefully written. His lasting and most enduring scholarly accomplishment was the publication of a prize-winning, monumental two-volume reference work on *Social Service Organizations* in the Greenwood Press *Encyclopedia of American Institutions*. Its appearance was all the more remarkable in view of Peter's hospitalization during both the research and writing.

Peter's love of New York City was one of his prized idiosyncracies. He gloried in riding his bicycle during rush hour, doing without a car, and discovering Chinatown's most obscure but delicious restaurants. Nothing gave him more pleasure than taking his nephews to a Yankee baseball or Ranger hockey game. He brought joy to those whose lives he touched, and he will be greatly missed but not forgotten by his friends who loved him dearly.

EUGENE M. TOBIN  
*Hamilton College*

GEORGE GORDON WINDELL, professor of history at the University of New Orleans since 1969, died on July 3, 1979, in Geneva, Switzerland. He and his wife

were en route to Innsbruck, Austria, where he was to have participated in the university's Innsbruck Summer School Program. After falling ill in Spain, he was hospitalized upon reaching Geneva, and died following emergency surgery to repair a cerebral aneurysm.

Born in Seattle, Washington, in 1920 to George A. and Grace E. Windell, he grew up in Kansas City, Missouri. In 1940, he took an A.B. degree with distinction at the University of Tulsa, and in the following year he received a master's degree in history from the University of Missouri. Serving the United States Army from 1942 to 1945, he participated in five European campaigns as a member of the 45th Field Evacuation Hospital.

He resumed his graduate studies at the end of World War II, first at the University of Missouri, where he was also instructor of history, and then at the University of Minnesota, where, in 1953, he earned his doctorate under the tutelage of Lawrence D. Steefel. He joined the faculty of the University of Delaware in 1948, and he taught there for twenty years, as well as serving as president of the local chapter of the American Association of University Professors and participating in other faculty and cultural affairs.

During his tenure at the University of New Orleans, Professor Windell was coordinator of graduate studies in history and served on many university, collegiate, and departmental committees. He also taught regularly in the University's European summer program, in addition to being visiting professor at Cornell and Tulane Universities. Among his other activities, he was noted in New Orleans as the producer-host of "Opera Hall," a weekly broadcast of WWNO, the local public radio affiliate.

An outstanding historian, whose fields were German and European intellectual history, Professor Windell authored *The Catholics and German Unity* (1954), as well as numerous articles and reviews which appeared in major scholarly journals. His honors included awards for postdoctoral research in Germany, a Fulbright-Hays Fellowship in 1954, and a grant from the American Philosophical Society in 1962. He held memberships in several scholastic honor societies, as well as the American and Southern Historical Associations.

At the time of his death, he was preparing the manuscript for a second book, which was to have examined the relationship between romanticism and nineteenth-century science. He had also undertaken research into the careers of nineteenth-century German feminists.

One of the most popular teachers at the University of New Orleans, Professor Windell introduced many neophytes to the discipline of history. His keen mind and sharp wit attracted both undergraduate and graduate students to his courses, and

a number of his former students at Delaware and UNO have become practicing historians.

Professor Windell is survived by his wife, the former Marie Elizabeth George. He was memorialized by colleagues, friends, and students at the Episcopal Church of the Holy Comforter, New Orleans, on August 27, 1979. His ashes were interred at Arlington National Cemetery, where his father, a

Navy veteran of World War I, and his mother lie buried.

The department of history at the University of New Orleans has established a scholarship fund as a memorial to Professor Windell.

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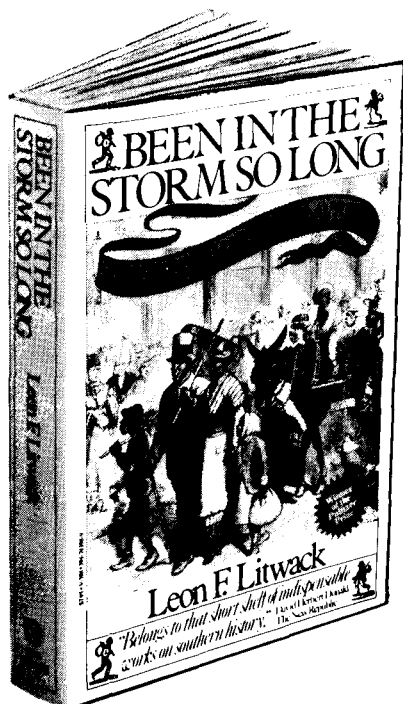
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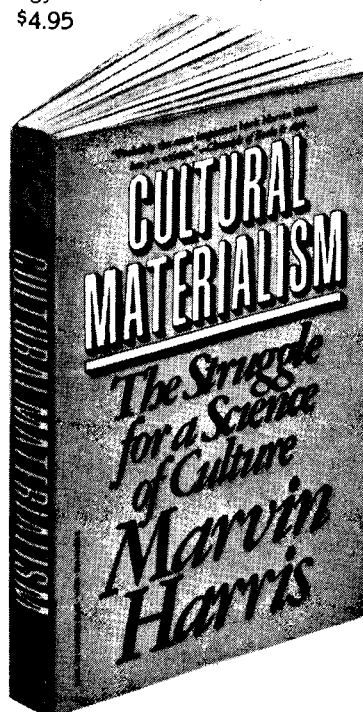
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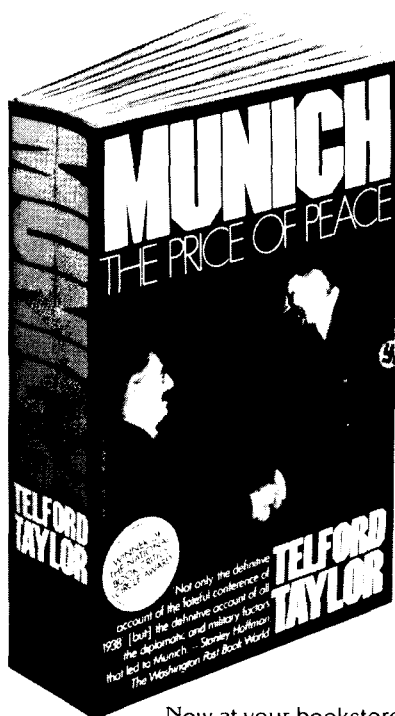
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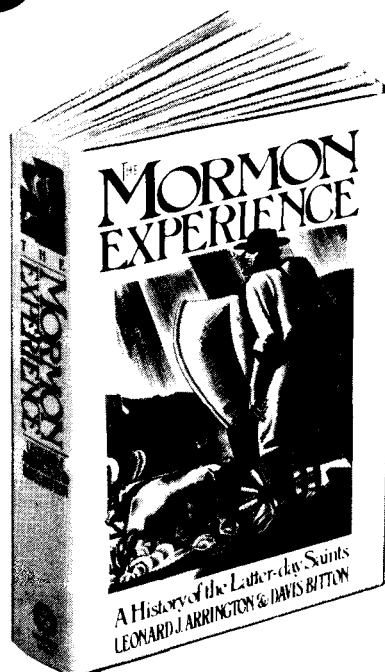
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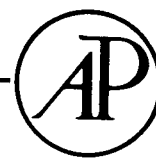
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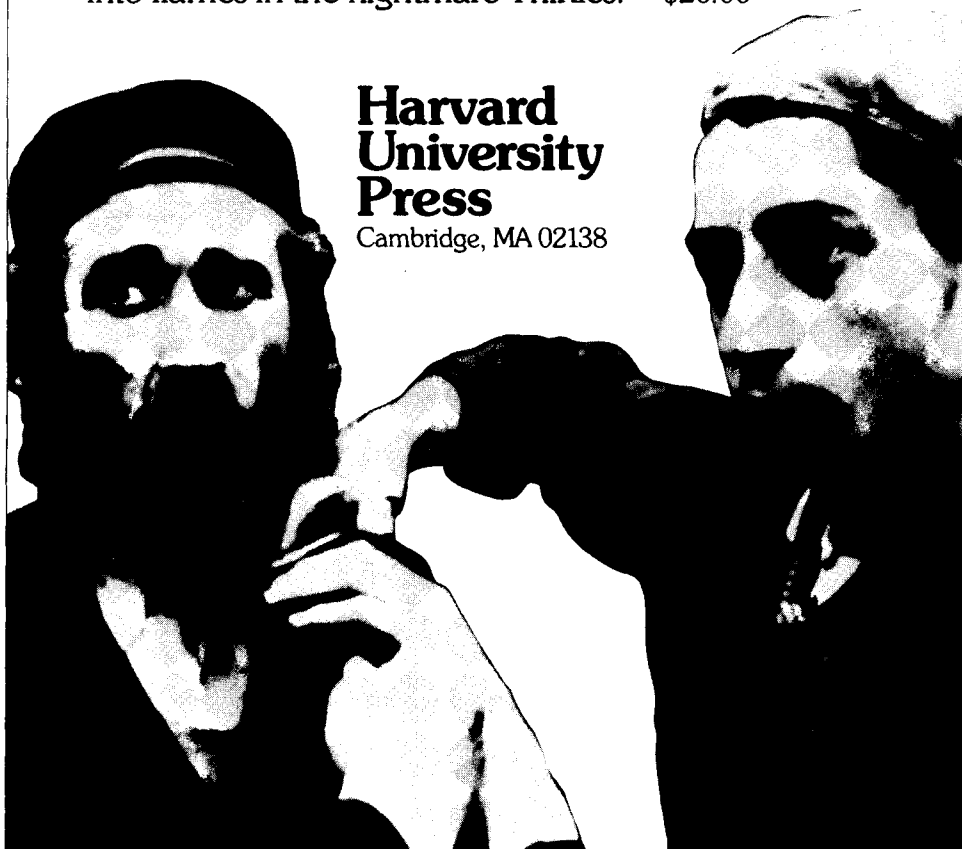
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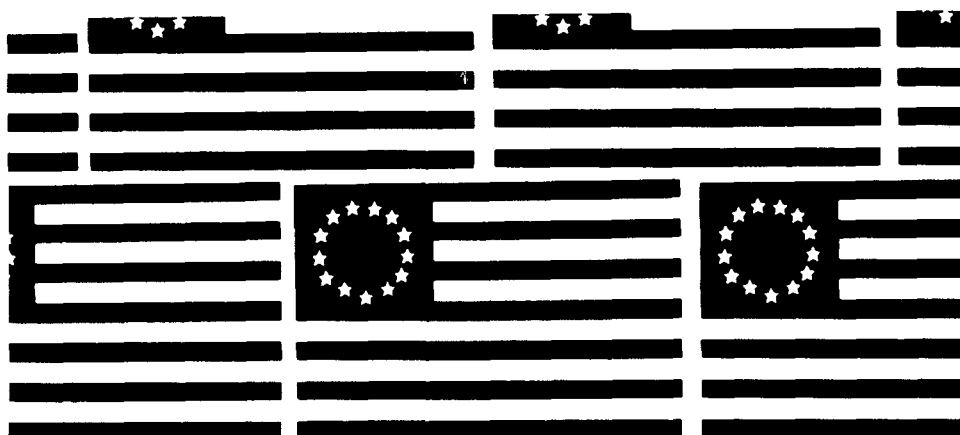
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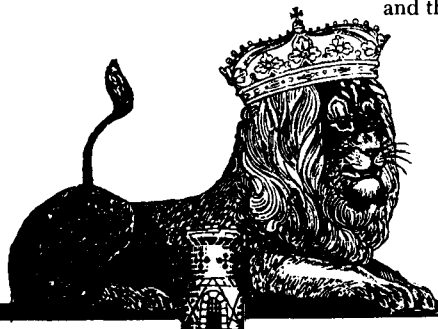
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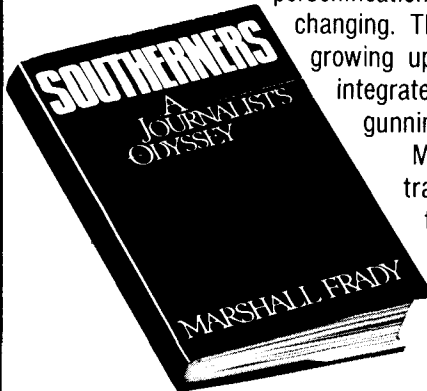
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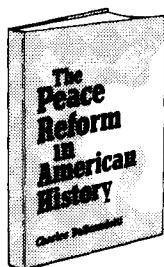
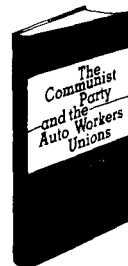
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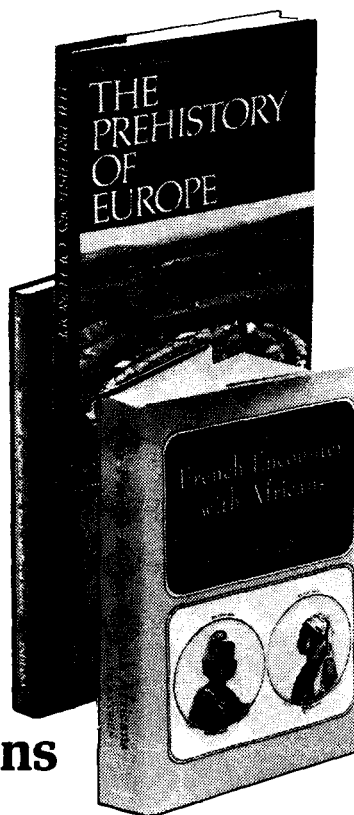
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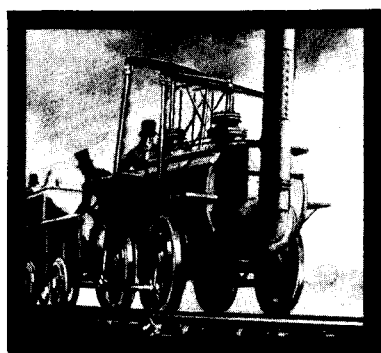


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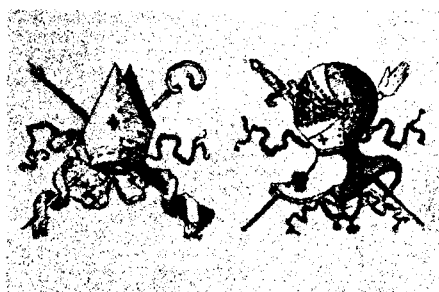
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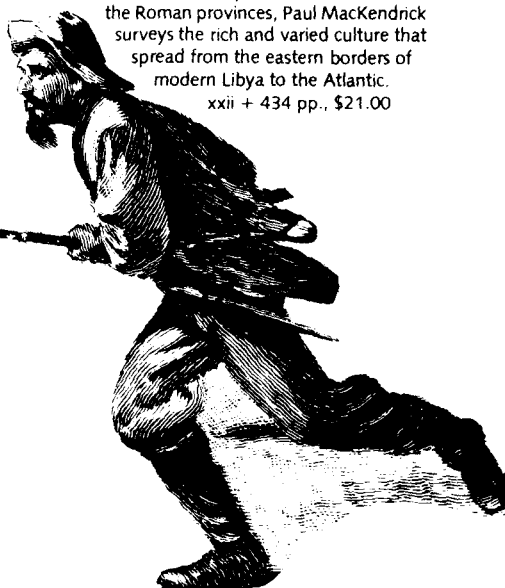
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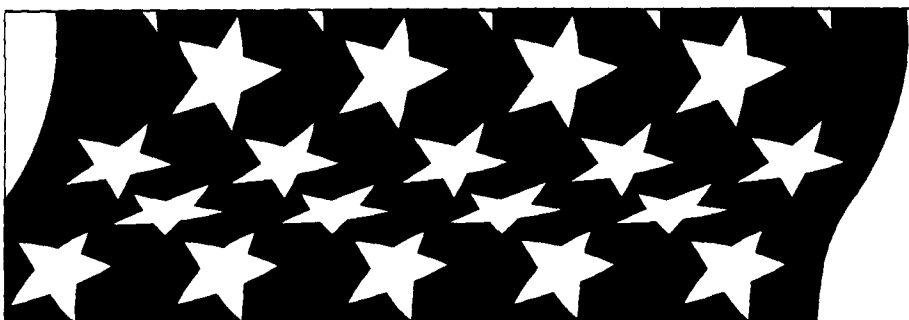
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